AN ACCOUNT OF

EXPERIMENTS

MADE AT THE

PANTHEON,

On the NATURE and USE of

CONDUCTORS:

TO WHICH ARE ADDED,

Some NEW EXPERIMENTS with the

LEYDEN PHIAL.

Read at the MEETINGS of the

ROYAL SOCIETY.

LONDON:
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To the KING.

SIR,

YOUR MAJESTY, in conſequence of the accident from lightning, that happened to one of the buildings at *Purfleet* in *May* laſt, having been graciouſly pleaſed to intimate the propriety of making ſome further experiments, to aſcertain the moſt probable method of preventing ſuch accidents for the future; and having alſo condeſcended to be preſent at the exhibition of thoſe experiments at the *Pantheon*; I have preſumed to addreſs to YOUR MAJESTY this faithful and circumſtantial account of what was there attempted, together with ſome obſervations thereupon, as an humble teſtimony of my duty and gratitude for the great honour conferred upon me.

How far I may have ſucceeded in theſe my zealous endeavours to aſcertain the moſt proper conſtruction for conductors, is, with the greateſt deference, ſubmitted to Your MAJESTY and the public. And whatever conſequences may be derived from theſe experiments, I am happy in the thought of having done every thing in my power, with the utmoſt candour and impartiality, to inveſtigate truth in a queſtion of real advantage to ſcience, and of ſuch importance to the public, as ſeems, in my humble opinion, worthy the attention of the ableſt philoſophers.

I am, SIR,

Your MAJESTY's moſt faithful

Nov. 12, 1777. and moſt dutiful ſubject,

BENJAMIN WILSON.

NEW EXPERIMENTS AND OBSERVATIONS ON THE NATURE AND USE OF CONDUCTORS,

BY

BENJAMIN WILSON, F. R. S.

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THE experiments I propoſe to give an account of in this paper were made in conſequence of the accident from lightning, which happened to one of the buildings belonging to his Majeſty's magazine of gunpowder at Purfleet, on the 15th of May laſt.

Soon after that event, an official and particular account having been ſent by the Board of Ordnance to the Royal Society, a committee of the members was immediately appointed, to examine the damage done to that building, and afterwards to make a report of the ſame.

When that report was laid before the Society, I thought it my duty in particular to ſtand forth, and offer ſome objections in writing to the uſing pointed conductors at Purfleet, or indeed any where elſe.

This public proceeding was, I apprehended, the more neceſſary, as I had, upon a former occaſion, in the year 1772, declared my *diſſent* from the report then made by the committee, who had recommended ſharp pointed conductors, for that magazine, to be fixed ten feet higher than the reſpective buildings.

But notwithſtanding I had read the paper alluded to above, I did not apprehend that my duty was fully diſcharged, without trying other methods of having ſo ſerious and intereſting a ſubject further inquired into.

I had

I had the ſatisfaction, ſoon after, to meet with ſufficient encouragement to induce me to conſider of ſome experiments, which might make the ſubject in diſpute more intelligible.

The plan I conceived to be the moſt proper for this purpoſe, was to have a ſcene repreſented by art, as nearly as might be ſimilar to that which was ſo lately exhibited at Purfleet by nature.

To carry a deſign of that kind into execution, it was neceſſary that attention ſhould be given to the ſeveral circumſtances concerned in the event at Purfleet.

The moſt material of thoſe circumſtances I apprehended to conſiſt in having a ſubſtitute for a *thunder cloud*, as it is vulgarly called, and large enough, or ſufficiently long, to admit of being charged with a conſiderable quantity of the matter of lightning by artificial means; and likewiſe, that this ſubſtitute ſhould admit of being eaſily moved, and with any velocity the experiment required: or, at leaſt, ſo as to equal the motion of a thunder cloud.

An apparatus, ſufficiently large for theſe purpoſes, could not conveniently be put in motion; therefore I propoſed to get rid of this difficulty, by moving the building itſelf, inſtead of the ſubſtitute; as that would anſwer the ſame end.

In order to obtain a conſiderable charge of artificial lightning, I propoſed to have one great cylinder covered with *tin-foil*, and a wire joined to one end of it, that ſhould, when extended properly, conſiſt of ſeveral hundred yards.

This idea leading to an expence too conſiderable for an individual, I preſumed to hope for other aſſiſtance.

Upon an humble repreſentation of theſe matters, his Majeſty, who is always diſpoſed to promote every purſuit which tends to the advancement of ſcience, and the good of the public, moſt graciouſly condeſcended to encourage the undertaking. And by the favour of the right honorable and honorable Board of Ordnance, I was immediately enabled to carry the intended plan into execution.

Very

Very ſoon after this encouragement, I procured correct drawings of the building called *The Board-houſe,* at Purfleet: from theſe an exact model was made; excepting the windows and doors, and thoſe were omitted, becauſe they were immaterial upon this particular occaſion.

In this model, a ſtrict attention was paid to thoſe parts of the building where metal had been introduced; ſuch as the hips, and gutters of the roof, and the ſeveral ſpouts to carry off the water. And as the north-eaſt corner of the houſe was the part that ſuffered by lightning in May laſt, particular attention was paid to the two cramps at that corner, and the two ſpouts on the north-ſide. Theſe cramps, in the model, were made of ſmall wire, that bore nearly an exact proportion to thoſe in the building itſelf; not only in regard to length and thickneſs, but alſo their diſtance from each other, and from the turning up of the lead appertaining to the gutter.

The two ſpouts were repreſented each by a thick wire, the ſhorter of which communicated

cated (at the bottom) with a ciſtern. This ciſtern reſting upon two wooden pillars, or poſts, about one foot and a half in length at Purfleet, the ſame circumſtances and proportions were attended to, and made to correſpond exactly in the model.

The other wire, in conformity to, and nearly in proportion with, the other ſpout at the Board-houſe, deſcending from the gutter for about ſeven inches, was there bent almoſt at right angles, and then continued on for twelve inches and three quarters, in a line nearly horizontal, till it reached within two inches (or little more) of the ſhort wire: after which it was bent again almoſt at a right angle, and then lengthened out to the bottom of the model, from whence it communicated, by another wire, with a pump or well, in another part of the houſe.

This kind of communication was neceſſary, becauſe, conſiſting of metal, it, in that reſpect, was ſimilar to the communication at Purfleet.

Beſides the two cramps mentioned above, another parapet was made to put on occaſionally,

ally, which contained all the cramps: these were properly fixed therein, and at their proper diſtances from each other. And the Royal Society having thought proper, ſince the accident, to order that a metallic communication ſhould be made between the cramps upon the parapet, quite round the building, as a better ſecurity from ſuch accidents for the future, care was taken to make a ſimilar connection with the wire cramps, by means of ſmall ſlips of tin-foil that were paſted upon the parapet of this model.

On the top of the roof in the middle, conductors of different lengths and terminations were occaſionally put, juſt as the experiments required.

The ſcale from which this model was made, when compared with the houſe, was one third of an inch to a foot.

In regard to the wood of which the model was made, I took care that it was well baked and ſoaked, whilſt hot, in drying oil, before the ſeveral parts were joined together, that it might be the more ſimilar to the bricks and other

materials

materials of the building itſelf, in the power of reſiſting the paſſage of the fluid, whenever any attack thereof ſhould be made. For brick, ſtone, dry lime, &c. had been obſerved (many years ago by Mr. Delaval and others) to reſiſt the paſſage of this fluid very conſiderably.

In order to move this model with the velocity required, it was neceſſary to have a frame of wood, of ſuch a length, as would ſuffer the model, with the pointed conductor upon it, to be out of the reach, or influence, of the charge contained in the cylinder; both at its ſetting off, and when it had arrived at the end of its journey.

To this frame, two upright poſts of wood, ten feet and a half long, were fixed at the further end, and at a diſtance from each other equal to the width of the frame. Upon the top of theſe poſts, and in the middle between them, were fixed two wheels of different diameters upon the ſame axis. The larger took the line that was propoſed to draw the model, and the leſſer another line ſuſpending two weights which regulated its motion. For after

the heavier weight had deſcended ſo far as to bring the model directly under the ſubſtitute, it was then checked; but the leſſer weight continuing to deſcend, the model moved forward with its acquired velocity, joined to the power of the leſſer weight. And that the remaining motion might at laſt be overcome, without ſtriking againſt the two poſts, ſome narrow ſlips of cloth (ſeven feet long) were nailed upon the frame, and in thoſe parts over which the model was to paſs before it reached the end. This model moved like a ſledge, by means of two ſlips of wood that were fixed at the bottom, which ran in two grooves that were cut along the frame from end to end. And the line which drew the model along was fixed very near the center of reſiſtance.

To conſtruct the ſubſtitute for a cloud, I firſt joined together, in *fifteen lengths*, the broad rims of one hundred and twenty drums, (merely to have them portable) by means of wood cut into long ſlips, which were fixed on the inſides thereof. But as thoſe drums were not accurately of a ſize, the ſeveral joinings were co-

vered over with cloth, and pasted down, to make the surface throughout more even. After this, the whole number were properly covered with tin-foil, excepting eight; for these being brass, required to be covered only at their joinings. All these drums together formed a cylinder above one hundred and fifty-five feet in length, and above sixteen inches in diameter.

The whole cylinder was made in four separate parts; three of those parts could easily be made to communicate, or not, with each other: the fourth being brass, was reserved for a different purpose. The several ends of those four parts were closed up with board, rounded off at the edges in every part, and covered with tin-foil likewise.

This great cylinder (consisting of the three parts) was suspended about five or six feet from the floor, by silk lines; and formed a curve in the room, something like an horse-shoe; one end of which hung over the middle of the long frame, on which the model was proposed to move; the other (which I call the further end) was joined occasionally to the end of a long

wire,

wire, that was ſuſpended through the whole ſpace of the room. And leſt the ſeveral atmoſpheres round this wire, in its charged ſtate, might, in conſequence of the unavoidable returns of the wire, interfere too much with each other, it was ſuſpended in ſuch a manner (by ſilk lines alſo) that each length was five or ſix feet from its neighbour: and thoſe that were ſuſpended neareſt to the great cylinder hung at the ſame diſtance from it. The remote end of this long wire hooked-on occaſionally at the end of the braſs drums, which made a ſeparate cylinder (the fourrh part alluded to above) about ten feet in length: this was ſuſpended likewiſe by ſilk lines, and about ſix feet from the floor, but in ſuch a manner, that the fartheſt end thereof from the wire was within nine or ten feet of the great cylinder.

The long wire with the great cylinder and braſs drums made the whole of the ſubſtitute for a thunder cloud, when they were properly charged.

The machine employed to charge this apparatus, conſiſted at firſt of two large glaſs cylin-

ders that were turned by one wheel. But as the friction arising from the two together rendered it difficult to work them, and the advantage gained from both in the charge itself was found to be not so considerable as might reasonably be expected, one of them only was made use of in the following experiments. The place where this machine charged the great cylinder, was about ten or eleven feet from its nearest end. It was found expedient to be provided also with another machine; but this was employed only upon particular occasions, and was generally placed at the further end of the great cylinder.

The floor of the room being of baked wood, it was necessary to have wires properly connected with the cushions of both machines along the floor, where they were joined to another wire, which communicated with the well, in order to conduct the fluid more readily than the baked wood admitted of.

The whole of this apparatus, so contrived, was disposed in the great room of the *Pantheon*, by the favour of the *Proprietors*, who,

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having heard that a large apartment was wanted, in which to ſhew before the Board of Ordnance, and the Royal Society, theſe experiments, were pleaſed to honour me with a very polite letter, offering the uſe of that elegant building for the purpoſes intended.

Iſt EXPERIMENT.

The model, with a pointed conductor upon it, (which, in degree of ſharpneſs, was nearly equal to that of a common darning needle) being placed directly under the nearer end of the great cylinder, ſo that the diſtance betwixt the point of the conductor and cylinder was little more than four inches, the machine was then put into motion. When, after two turns of the wheel, or thereabouts, a ſmall ſtream of light appeared at a little interval, between the top of the longeſt thick wire which repreſented the bent ſpout, and its little ciſtern next to the gutter, where the metallic communication was purpoſely interrupted. This ſtream continued to be viſible, though the model was moved along

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the frame from its fixed ſtation to more than the diſtance of 43 inches.

IId EXPERIMENT.

When a conductor of the ſame length with the former, but rounded at the end, and no more than three tenths of an inch in diameter, was put in the place of the pointed one, every other circumſtance continuing the ſame, the ſmall ſtream of light appeared again: but upon moving the model a little beyond the diſtance of 16 inches, it totally diſappeared.

Firſt obſervation. By the firſt experiment it was manifeſt, that the point acted upon the charge all the time the model was moving through a ſpace equal to 43 inches; and conſequently was, all that time, diminiſhing the charge in the great cylinder. On the other hand, the ſecond experiment ſhewed, that the rounded end acted upon the charge only whilſt the model moved through a ſpace equal to 16 inches. And, from the two experiments compared, it appears, that a charged body is exhauſted

haufted of more of the fluid by a pointed, than by a blunted, conductor.

IIId EXPERIMENT.

If in the place of the long rounded conductor a fimilar one was put, but about one-fifth of the length, whilft the model ftood directly under the great cylinder as before, the charge contained therein produced no appearance of light whatfoever.

IVth EXPERIMENT.

But when a pointed conductor of the fame length with the laft was put in its place, the fmall ftream of light appeared, and continued vifible all the time the model was moved through a fpace equal to 18 inches and a half.

Second obfervation. Thefe laft experiments, compared with the former two, fhew that a rounded conductor, little more than one foot and a half above the higheft part of a building, receives a far lefs quantity of the matter of lightning from a cloud fully charged therewith, than a pointed conductor placed ten feet above a building,

a building, circumſtanced alike in every other reſpect. Nay, a pointed conductor of the ſame length with the ſhort one that was rounded, appears, from theſe experiments, to collect a greater quantity of the fluid, than even the long conductor with a rounded end.

The difficulty of meaſuring exactly the effects, when the great cylinder was charged, was ſo great, that after a variety of endeavours to aſcertain the quantity of the charge remaining, when different terminations and different lengths of conductors were employed, I was obliged at laſt to have recourſe to the ſenſe of feeling, uncertain as it is in many caſes, to determine the different effects, occaſioned by the interpoſition of theſe different terminations.

Vth EXPERIMENT.

On repeating the firſt experiment (that is, with the long pointed conductor upon the model) with ten turns of the wheel only, the charge remaining in the great cylinder was immediately received on the hand; the ſenſation it occaſioned was little more than perceptible.

VIth EXPERIMENT.

VIth EXPERIMENT.

But upon repeating the ſecond experiment, (that is, with the long rounded conductor upon the model) with ten turns of the wheel alſo, the charge remaining was taken, but the ſenſation in this caſe was increaſed conſiderably.

VIIth EXPERIMENT.

When the third experiment was repeated, (that is, with a rounded conductor five times ſhorter than the laſt) and with the ſame number of turns, the ſenſation was obſerved to be full as violent, as if no ſuch metallic interpoſition had been preſented to it.

VIIIth EXPERIMENT.

Upon repeating the fourth experiment, (that is, with the pointed conductor equal in length to that in the laſt experiment) and with ten turns of the wheel alſo, the ſenſation was not near ſo conſiderable, for it ſeemed ſomething leſs than what was experienced with the long rounded conductor in the VIth experiment.

Third obſervation. The ſeveral effects obſerved in the four laſt experiments agreeing ſo exactly with thoſe in the four firſt, prove, at leaſt ſo far, that rounded ends, in theſe caſes, received the fluid leſs readily, and in leſs quantity, than points.

But, before other experiments are related, it may be proper to take notice of what paſſed at the ſhort ſpout upon the model during the making of the four firſt experiments; becauſe the conſequences to be drawn from thence are very material.

IXth EXPERIMENT.

In the firſt experiment (when the long pointed conductor was put upon the model, and whilſt the wheel was turning) a very ſmall ſpark might be taken from the ſhort ſpout; but if the hand, or a wire, was applied, which communicated with the well, and continued in contact with the ſhort ſpout, or, if it was connected with the long ſpout, the ſmall ſtream of light ſeen before at the top of that ſpout, now ceaſed.

Xth EXPERIMENT.

But this was not the caſe when the IIId experiment was repeated, (that is, with the ſhort rounded conductor) for this, by reaſon of its greater diſtance from the cylinder, and the nature of its termination, did not draw a ſufficient quantity of the charge from the great cylinder to cauſe the leaſt appearance of a ſtream of light as in the former caſe.

XIth EXPERIMENT.

The effect, however, was different, when a pointed conductor of the ſame length as in the IVth experiment was made uſe of; for then the ſhort ſpout was charged nearly in the ſame manner as in the firſt experiment, and the ſtream of light at the top of the bent ſpout diſappeared the inſtant a communication was made from the ſhort ſpout to the earth.

Fourth obſervation. The two poſts of wood upon the ground, which ſupported the ciſtern at the bottom of the ſhort ſpout, were therefore the true cauſe of theſe effects taking place in the ſhort ſpout, by preventing a communication

with the earth, and hindering the fluid that was conſtantly charging the ſhort ſpout from diſcharging itſelf properly into the earth.

It may now be proper to take notice of other experiments, and alſo of two other circumſtances that ſeem to be of conſiderable conſequence in this inquiry.

One of theſe circumſtances reſpects the motion of the model, inſtead of that of the cloud; and the other, the quantity of the fluid contained in the great cylinder when properly charged.

As to the former of theſe, it appears from obſervations, that clouds, in a very high ſtorm, frequently move at the rate of eighty miles in an hour; and, with a moderate wind, about twenty miles. Now when the clouds move at the rate of four or five miles an hour, the wind which occaſions that motion is, I apprehend, in general, little more than what is uſually ſaid to be ſenſible. For it has been frequently obſerved, that a traveller on horſe-back muſt ride with a pretty good ſpeed, to keep pace with the ſhadow from a cloud, when it chances to move

N. B. In the Philoſophical Tranſactions there is an account of a ſtorm that was computed to move at the rate of three miles in a minute.

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in the direction of his journey, even though the wind at ſuch a time can ſcarcely be ſaid to blow.

Now a thunder cloud, as it is uſually called, moves, as I apprehend, in many caſes, at leaſt, with a much greater velocity; and though there may be a few inſtances where ſuch clouds move with a leſs, I think, if the motion of them be put, in general, at four or five miles an hour, it ought to be conſidered, in this caſe, as a very moderate computation. For theſe reaſons, the weight to draw the model was adjuſted to that velocity.

In regard to the quantity of the fluid required to charge the great cylinder, I found, from many experiments, that twenty uniform, and rather briſk, turns of the wheel, were the moſt favourable for the following experiments; becauſe, half a turn in twenty made a far leſs difference in the charging, than half a turn of the ſame wheel, when eight or ten turns only were required, and with nearly the ſame uniform velocity.

In obſerving this laſt rule, proper allowance was made for the different ſtates of the air, the ſtate of the glaſs itſelf, and that of the cuſhion or

or rubber, which excited the fluid by a proper preſſure upon the glaſs, joined to other circumſtances. The moſt material of which conſiſted in the cuſhion's having a free communication with the earth and glaſs cylinder, in that part where the friction was applied; and alſo a ſimilar communication between the great cylinder and the oppoſite ſide of the glaſs. Now, becauſe three pointed wires conducted the fluid readily from the glaſs to the great cylinder, I apprehended it was proper to have three ſuch points within the cuſhion itſelf to conduct the fluid as readily from the earth to the glaſs. For thoſe points communicated by a wire with the well.

It will appear, that the effects produced by this artificial method of charging the cylinder, muſt be very different from thoſe produced in nature.

Suppoſe a thunder cloud coming already charged, the lightning from this cloud ſtrikes at Purfleet; the ſame cloud, paſſing afterwards over other places, will ſtrike the earth again and again, without any apparent diminution of the quantity of lightning contained therein, as hath been frequently obſerved. But the caſe is not the ſame

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with the great artificial apparatus, becauſe we not only charge it by degrees, but when a ſtroke is taken from it, the greateſt part of the charge by far is at that inſtant taken out of it. And therefore we are conſtantly under the neceſſity of renewing the charge before ſuch another ſtroke can be taken.

Beſides, thunder clouds, from their nature, and a variety of circumſtances accompanying them, never aſſume the ſame ſhape and ſize: neither are they always at the ſame diſtance from each other. And yet we are told, that they have been obſerved to ſtrike from one to another at different diſtances, juſt as they happen at the time to be circumſtanced.

For all theſe reaſons, I thought it was proper to make particular experiments in different caſes, where ſome of the circumſtances varied. And becauſe a ſingle cloud, after it hath ſtruck any one object, ſometimes continues to diſcharge vaſt quantities of lightning, I propoſed to begin the experiments with the great cylinder only.

XIIth EXPERIMENT.

XIIth EXPERIMENT.

The model (furniſhed with the wire of communication, and with the longeſt pointed conductor upon it) being properly placed upon the long frame, and held there in readineſs to be drawn forward by the line and weight at the other end, the great cylinder was charged by 20 turns of the wheel. On letting go the model, and almoſt at the inſtant before the point came under the center of the cylinder, it was ſuddenly ſtruck with the matter of lightning, and frequently ſooner. The leaſt diſtance of the point from the great cylinder, when this ſtroke happened, meaſured nearly five inches. The quantity of charge that remained in the cylinder was very little to the ſenſe of feeling, though taken immediately after the ſtroke happened.

XIIIth EXPERIMENT.

On putting into the place of the pointed conductor one of the ſame length, that was rounded at the end, and without any other change of circumſtances, the wheel having been turned the ſame number of times, I ſuffered the model to

paſs:

pass: but the rounded end, in this case, was not struck. However, the instant after it had passed, the quantity of the charge that remained in the cylinder was taken, and in the same manner by the hand; on the doing of which, the sensation was more violent than in the last experiment.

Fifth observation. From the two last experiments it appears, that though every circumstance was the same, excepting the different terminations of the two conductors, yet the pointed one only was struck; notwithstanding they were both of the same length, and passed at equal distances from the cylinder. From whence we collect, that the quantity of lightning discharged from the great cylinder into the point, when an explosion happened, was considerably greater than the quantity discharged into the rounded end, when there was no explosion.

At the first rise of a difference in opinion, respecting the proper termination and length for conductors, I was prevailed uopn by some learned members of the Royal Society, in the year 1764, to publish my sentiments upon that subject. Accordingly, in a letter addressed to the

Marquis of Rockingham,* (after ſtating ſeveral reaſons againſt the uſe of points, as I ſuppoſe they *invited* the lightning) I there recommended that conductors ſhould not only be rounded at their ends, but be made conſiderably ſhorter than thoſe which Dr. Franklin contended for, and indeed ſhould not exceed the higheſt part of the building. In the following experiment, however, I did not place the pointed conductor below, nor upon a level with the higheſt part of the building; but above it, even one-third of the length of that in the XIIth and XIIIth experiments.

XIVth EXPERIMENT.

The model being thus furniſhed, and every thing elſe put exactly into the ſame circumſtances as in the XIIIth experiment, the great cylinder was charged by 20 turns of the wheel. Upon letting go the model, it paſſed the cylinder at the diſtance of ſeven inches, without being ſtruck: but the charge that remained in the cylinder at the inſtant after the model had paſſed it, was ſo conſiderable, that there appeared no ma-

* Phil. Tranſ. Vol. LIV.

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terial difference whether the model thus circumſtanced was ſuffered to paſs or not.

Sixth obſervation. This laſt experiment ſhews, that a thunder cloud may paſs a conductor ſo circumſtanced without its being ſtruck, or ſuffering the leaſt injury, when it will not in other circumſtances; that is, when the conductor is pointed, and raiſed ten feet above the building.

XVth EXPERIMENT.

On repeating the XIVth experiment, but with a rounded conductor, which was three-fourths of the whole length of that in the XIIth experiment, (all other circumſtances remaining the ſame) and after charging the cylinder by an equal number of turns, it paſſed alſo without being ſtruck. In this caſe, the remaining charge in the cylinder was ſomething leſs than in the laſt experiment.

Seventh obſervation. This is a further inſtance of the advantage derived from having rounded terminations of a given length upon a building, compared with pointed ones, that are only two or three feet longer.

Having ſo far experienced the different effects of different terminations in the preceding experiments, it may be proper to mention another experiment, where the rounded end was ſtruck. But as its diſtance from the cylinder, at that inſtant, was only one quarter of an inch leſs than the diſtance at which the point had been ſtruck, I ſhall take no further notice of it in this place; becauſe, the ſeveral ſtriking diſtances of thoſe different terminations will (in another part of this paper) be correctly aſcertained, in a different manner, by other experiments.

We are now to examine, whether the ſtroke by lightning, which happened at Purfleet, fell firſt upon the corner of the building where the cramps were affected, as hath been repreſented; or whether it did not fall upon the point of the conductor itſelf? And if it fell upon the point, how could it poſſibly affect thoſe cramps, as they had no metallic communication with the main conductor which extended from the top to the bottom of the houſe, and of which the gutter below the parapet, that was neareſt to the cramps, made a part?

It muſt be remembered, that though in attempting an experiment of this kind, we are provided with an apparatus greater perhaps than was ever conſtructed before; yet, great as it is, it bears but a very ſmall proportion to that which nature makes uſe of. On this account, we muſt expect but very faint appearances compared with thoſe which are produced by a thunder cloud. But before we relate theſe appearances, it may be proper firſt to ſee what effect the charge itſelf has upon the model, without any conductor upon it.

XVIth EXPERIMENT.

Upon charging the great cylinder as before, that is, by 20 turns of the wheel, and when the model, without any conductor upon it, was let go, there was no exploſion in or on any part of it, during the time of its paſſing by the cylinder; notwithſtanding the model itſelf was properly connected with metal from the top of the roof to the bottom of it, and afterwards to the well.

XVIIth EXPERIMENT.

But when the experiment, with the long pointed conductor upon the model, was repeated, and after the wheel had been turned an equal number of times, the point was ſtruck as it paſſed the cylinder: and, at the ſame inſtant alſo, a very ſmall ſtream-like exploſion appeared between the two cramps at the corner of the model, darting as it ſeemed from one to the other, in a direction that was rather particular. This ſtream-like appearance, I apprehended, was nothing more than the effect of a ſmall exploſion in conſequence of the motion of the model.

Mr. Wyat, who is well acquainted with this part of philoſophy, and to whom great obligations are due from me, for his very friendly aſſiſtance in the whole of this undertaking, having obſerved this appearance ſeveral times, was curious to confirm the fact, leſt the prejudices of viſion, &c. might poſſibly deceive him.

The method he took upon this occaſion, was to obſerve an appearance of light along that hip of the roof which was next to the corner ſtruck; and

and whether the direction of that appearance was different from the direction of the leffer appearance he had obferved at the corner between the two cramps. Thefe appearances he endeavoured to afcertain by means of a paper tube, blacked on the infide, that was laid upon a ftand of a proper height, fo that when he looked through it, a pin, being ftuck upright at the further end thereof, coincided with the part in the model he was to examine; that is, when the model ftood directly under the cylinder. Being thus circumftanced, and when the corner of the model (during its motion) paffed the pin, he faw the direction of the light along the hip next the corner, and alfo between the cramps at the fame time; and was pofitive that the direction of the light appeared to be downward from the roof, and, as he thought, horizontal between the two cramps. Thefe appearances were obferved by others afterwards, exactly as Mr. Wyat had before defcribed them.

Eighth obfervation. By the XVIth experiment, it appeared, that the corner of the model, where the two cramps were inferted, paffed fafely by the

the charged cylinder, without affording even the leaſt luminous effect; and conſequently prove, that in ſuch circumſtances the two cramps could not poſſibly be ſtruck, becauſe the charge in the cylinder remained the ſame, or very nearly ſo, after the model had paſſed.

Ninth obſervation. But in the XVIIth experiment, the pointed conductor was fixed in its place upon the model, juſt like that at Purfleet; when not only the point was ſtruck, as the model paſſed the cylinder; but, at the ſame inſtant, a ſmall exploſion was ſeen between the two cramps at the corner. That this light between the cramps aroſe from a different cauſe than what had been ſuggeſted by the ſecond Purfleet committee, appeared from ſome circumſtances accompanying that effect. For example; theſe cramps had no connection with the gutter or ſpouts next them, but were quite ſeparate, and at the diſtance of ſix or ſeven inches from any metallic communication. And it is well known to philoſophers, that lightning always paſſes where it meets with the leaſt reſiſtance; they alſo know, that the leaſt reſiſtance in the

preſent

present instance must have been along the conductor at the top to the hips, gutters, and spout, and so on to the wire at the bottom to the well.

According to this law, the cramps themselves, then, were not properly circumstanced to receive the fluid as it passed to the earth, on account of the metallic communication, described above, being interrupted more than six inches.

Another cause therefore was necessary to explain the appearance; I mean that which is called the *lateral* effect; a term lately adopted, in consequence of an experiment I made near 30 years ago with the Leyden phial (which experiment has since been improved upon by Dr. Priestley): and though a charged glass (for such I call the Leyden phial) is by no means similar to the great cylinder when charged, for reasons that have been already published*; yet, in the course of these experiments, I apprehended the lateral effect produced with the charged glass might be considered in some respects as an illustration of the lateral effect which had been observed at the cramps.

* Further experiments and observations upon lightning, by Benjamin Wilson, published in 1774.

But, not to reſt this very material fact upon any doubtful opinion that may poſſibly be entertained reſpecting its exiſtence, other experiments will be produced, that may explain this matter more ſatisfactorily.

Before theſe experiments are related, it is proper to mention a material circumſtance, which hath hitherto been unnoticed.

At the time the accident happened at Purfleet, a great quantity of rain fell, by which the walls of the board-houſe, being made very wet, were diſpoſed to admit more readily the lightning paſſing upon the ſurface, though yet not ſo readily as a covering of metal would have done. This particular circumſtance of the rain will be attended to in its place.

But firſt it may be proper to ſhew the effects of a metallic communication.

To this end, I covered the top of the parapet quite round with tin-foil; and becauſe the copeing projected a little over the parapet, under which no rain could poſſibly get, I left a ſmall interval (proportional to it) uncovered with that metal. This interval was on the oppoſite ſide

of

of the model, which anſwered to the ſouth ſide of the building where the other ſpouts were fixed. Then from that interval I paſted a broad ſlip of tin-foil down to the bottom of the model, where having fixed a ſmall braſs ſtaple to hook a wire upon, I faſtened the other end of it to the wire which communicated with the well. At the ſame time care was taken to faſten another wire in the ſame manner, which communicated with the oppoſite ſide of the model. This wire was intended to anſwer the purpoſe of a ſpout in the building at Purfleet towards the ſouth.

Beſides theſe precautions, there remained another circumſtance to be attended to, reſpecting motion: for ſeveral gentlemen had obſerved, that clouds in a thunder ſtorm are not always in motion; or at leaſt not in the degree of motion which has been repreſented in the preceding experiment. To obviate that objection, I propoſed to repeat thoſe experiments, when the model was at reſt. In order to prepare the whole apparatus in the moſt proper manner for this purpoſe, it was neceſſary to attend to the circumſtances that are obſerved in nature.

For when a cloud comes ready charged, and ſtrikes another cloud with the matter of lightning, there muſt be a certain diſtance at which that effect muſt take place; and this we therefore call the greateſt, or the ſtriking diſtance. Now, to produce ſimilar effects, our artificial apparatus muſt conſiſt of two parts at leaſt, to repreſent ſuch clouds: and thoſe parts muſt be ſo diſpoſed as not to exceed the greateſt diſtance at which they will ſtrike, when the largeſt part, or ſubſtitute, is properly charged: nor muſt the diſtance between them be leſs, for reaſons that will appear preſently. And therefore, to make the experiment correſpond with nature, it will require ſome trouble to adjuſt this diſtance between the two ſubſtitutes. Now the diſtance between them will depend upon the quantity of the charge given: and the method to determine that diſtance may be found by removing one ſubſtitute from the other ſo far as not to cauſe any previous or partial exploſions before the great ſtroke happens; and when it does happen, it muſt not only ſtrike between the ſubſtitutes, but,

at

at the ſame inſtant, between the remote end of the leſſer ſubſtitute and the object oppoſed to it.

XVIIIth EXPERIMENT.

To each end of a ſlender ſubſtitute made of wood, about 11 feet in length, and ſomething leſs than one inch in diameter, was fixed a ball of the ſame matter. The larger of theſe meaſured 3 inches in diameter, and the leſſer 1 inch $\frac{9}{10}$. The exact meaſure of theſe balls was attended to the more, in ſome of the experiments, becauſe Mr. Nairne has given a deſcription of an apparatus in the Phil. Tranſ. vol. LXIV. part i. p. 87 and 88, to which this is nearly ſimilar. Having covered the whole of this ſlender ſubſtitute with tin-foil, it was then ſupported near the center by a ſlender frame of wood upon a pillar of glaſs, to adjuſt it to the height and diſtance required. The larger ball was then brought within one inch and a quarter (and ſometimes at a greater diſtance) of another ball, (one inch $\frac{9}{10}$ in diameter) that was covered with the ſame metal, and projected from the center of the nearer end of the

great

great cylinder by a kind of ſtem made of wood, and covered with tin-foil alſo, which was ſix or ſeven inches long. Being ſo prepared, the model was ſet upon a table, directly under the ball at the remote end of the leſſer ſubſtitute, with the pointed conductor upon it: and all the wires were properly connected, ſo as to make a free communication between the model and the well. Nothing now remained, but to put the machine in motion; when, after ten turns of the wheel, the point upon the model was ſtruck at the diſtance of four inches.

In the XIIth experiment, where the model was in motion, the point was ſtruck at the diſtance of five inches (nearly) from the cylinder. This difference of diſtance in theſe two experiments ſeemed to ariſe chiefly from a difference in the ſtates of the air; it being rather unfavourable when the XVIIIth experiment was tried.

XIXth EXPERIMENT.

However, ſome time after, when the ſtate of the air was very favourable, I repeated the laſt experiment,

experiment, and obſerved that the point was ſtruck at ſix inches and one quarter.

During this laſt experiment, a perſon, ſtanding upon the wire of communication, placed his finger in contact with the pointed conductor (near the bottom) at the time the ſtroke happened, when he received a blow uncommonly violent, inſomuch that he thought it exceeded greatly what he had ever experienced from the great cylinder only.

XXth EXPERIMENT.

I now repeated the XVIIIth experiment; and attending only to the two cramps at the corner, there appeared, at the inſtant when the point was ſtruck, a ſmall ſpark or exploſion between them: which clearly ſhewed, that the ſtream-like exploſion, obſerved in the XVIIth experiment, was only this ſmall ſpark accompanied with the circumſtance of the motion of the model.

Tenth obſervation. From what we have now experienced it appears, that thunder clouds, even at reſt, and that ſtrike each other at a given diſ-

tance

tance with the matter of lightning, occaſion the ſame phœnomena nearly, which a ſingle cloud produces when motion is introduced.

XXIſt EXPERIMENT.

When the diſtance between the two ſubſtitutes was made leſs in any degree than the greateſt ſtriking diſtance, (in proportion to its diminution the circumſtances were leſs ſimilar to thoſe in nature) it made a conſiderable difference in the effects; becauſe the fluid in theſe caſes paſſed more freely from the greater to the leſſer ſubſtitute: and the freer it paſſed into the latter, the nearer they approached to be one ſubſtitute. So that bringing the two ſubſtitutes into contact occaſioned the ſame phœnomena that the great cylinder did alone; that is, the rounded end would cauſe an exploſion at a conſiderable diſtance; and the point little or none, notwithſtanding it was brought almoſt cloſe to the ſubſtitute.

XXIId EXPERIMENT.

But if motion in this caſe was introduced, during the contact of the two ſubſtitutes, the

point

point was ſtruck at *nine inches* and *an half* diſtance from the ball. The motion employed upon this occaſion was by the hand only, which held a proper ſtand with the point upon it; and this point communicated by a wire with the well.

Eleventh obſervation. Now the nearer the two ſubſtitutes were brought together, the nearer they repreſented one cloud; and conſequently, as hath been obſerved before, the matter of lightning would paſs from one to the other in theſe caſes more readily, and without permitting ſo great an accumulation to take place as was experienced in the XIXth experiment, when the ſeparated ſubſtitutes ſtruck at the greateſt diſtance.

To occaſion ſuch a ſtroke, it is not only neceſſary to have an accumulation of the fluid, but that accumulation muſt be kept up, as it were, and increaſed ſuddenly, before a ſtroke in this caſe can poſſibly take place; as appeared manifeſtly by the motion introduced in the XXIId experiment, where the two ſubſtitutes were united: for the reſiſtance at a point being feeble, when therefore in this experiment a point was

ſuddenly brought towards the ball at the end of the great apparatus in charge, the whole of that charge muſt of conſequence ruſh towards the point, in an inſtant to diſcharge itſelf into the earth. This, I apprehend, was the true reaſon why we obtained an exploſion at the diſtance of nine inches and an half.

Having now gone through the experiments where points were introduced, we ſhall next relate the ſeveral experiments where other terminations were uſed. By this method of proceeding, we ſhall be able to form a proper judgment what kind of conductors are the moſt advantageous for ſecuring buildings, &c.

XXIIId EXPERIMENT.

On repeating the XVIIIth experiment, but with a rounded conductor upon the model, every other circumſtance continuing the ſame, and the model at reſt, the greateſt diſtance at which it was ſtruck (in conſequence of ten or eleven turns) was not more than three quarters of an inch.

Now if we compare this diſtance with that at which the point was ſtruck in the XVIIIth experiment,

experiment, the proportion will be found to be leſs than 1 to 5.

But this and the XVIIIth experiment, were repeated many times afterwards, for many days together, and when the ſtate of the air at each trial was very different; on which accounts the reſults of thoſe experiments varied as follow:

Sharp point.	*Rounded end.*
2 Inches $\frac{1}{4}$	$\frac{5}{8}$ of an Inch.
4	$\frac{1}{2}$
$3\frac{1}{4}$	$\frac{7}{8}$
$5\frac{1}{2}$	$\frac{4}{10}$
$6\frac{1}{2}$	$\frac{1}{2}$
2	$\frac{1}{10}$

Immediately after relating the XIXth experiment, mention was made of the ſenſation or blow that was received by a perſon who had brought his finger into contact with the pointed conductor at the time the ſtroke happened. The experiment was repeated here with the rounded end. But the ſenſation or blow received (as

well as the explofion) was confiderably weaker in this cafe.

XXIVth EXPERIMENT.

When the XXIIId* experiment was repeated, and the rounded end ftruck at three quarters of an inch, the fame kind of fpark appeared between the two cramps as in the XXth experiment when the point was ufed; but in this experiment, with the rounded end, the fpark at the cramps appeared confiderably lefs to every obferver.

XXVth EXPERIMENT.

Upon repeating the XXIft experiment where the two fubftitutes were brought into contact with each other, every other circumftance remaining the fame, the rounded end was ftruck at the fame diftance nearly as when a fpark was taken by a larger metal ball (fuppofe three inches in diameter) from any part of the great cylinder when equally charged. For in this cafe the two fubftitutes, being in contact, made in reality but one great fubftitute.

XXVIth EXPERIMENT.

XXVIth EXPERIMENT.

I now repeated the XXIId experiment, where motion was introduced; and without any other change of circumſtances than putting in the place of the point the rounded end. Upon this occaſion, as well as upon the former, the ſame perſon moved the ſtand with the rounded end upon it, and with the ſame velocity, (as near as he and others preſent could judge) but not before the connected ſubſtitutes were fully charged by an equal number of turns. The inſtant that the rounded end approached within a certain diſtance of the ball at the end of the leſſer ſubſtitute, it ſtruck; but the exploſion ſeemed inferior to that which the point occaſioned at the diſtance of nine inches and an half. In this experiment the diſtance between the rounded end and ball was not more than ſix inches and an half. From which it appears, that even in thoſe circumſtances the point was ſtruck at a greater diſtance than the rounded end, in the proportion of nine and an half to ſix and an half.

XXVIIth EXPERIMENT.

XXVIIth EXPERIMENT.

It has been obſerved, in a former part of this paper, that a great quantity of rain fell when the accident happened at Purfleet; as this circumſtance ſeemed to be material, it was proper to put the model into a ſimilar ſituation; and therefore, after removing the tin-foil upon the parapet, I waſhed the model all over with a ſponge; and, whilſt it continued in this ſtate, the machine was put into motion: after ten turns of the wheel, the point was ſtruck at five inches diſtance. In conſequence of which, a ſmall exploſion (more vivid than in the XVIIth experiment) appeared, not only between the cramps, but alſo another was ſeen, ſtill more vivid, at the inner corner of the parapet, neareſt to the cramps, darting, as it ſeemed, from the gutter up to the cramps.

XXVIIIth EXPERIMENT.

Upon paſting tin-foil upon the top of the parapet quite round the model, as had been done before, and moiſtening the inner part of the parapet

rapet down to the gutter, the ſtroke was again received by the point, when the ſame appearances were obſerved as in the laſt experiment.

XXIXth EXPERIMENT.

I then made a metallic communication between the top of the parapet down to the gutter; after this the experiment was repeated. And though the point was ſtruck at five inches diſtance, there was no ſpark whatſoever, either between the cramps or at the inner corner next to the gutter.

Twelfth obſervation. From theſe laſt experiments it appeared, that rain contributed to make the *lateral* effect greater at the corner, by forming a better communication between the cramps and the gutter, than the dry materials of brick and ſtone admitted of. But it alſo appeared, that when the communication between the gutter and cramps was rendered more perfect by a ſlip of tin-foil that was interpoſed between them, that *lateral* effect ceaſed at the cramps; becauſe a ſtill freer paſſage was made for the fluid to diſcharge itſelf through, not only along the ſlip of metal

communicating

communicating with the gutters and the tin-foil quite round the parapet, but alſo along the tin-foil down the ſide of the model, from whence it was conveyed by the wire to the well.

XXXth EXPERIMENT.

An objection having been made, that the wire communicating from the bottom of the model to the well, as it conſiſted of ſeveral diſtinct parts, occaſioned a reſiſtance at each of the junctions, and therefore conſtituted only an imperfect conductor; in order to try the validity of this objection, a new communication was now made from the model to the well, conſiſting of one entire wire, pointed at the end, with which I repeated the experiment of paſſing the model; and finding no ſenſible difference from the former reſults, I then, in order to apply a rounded end to this perfect communication, had the pointed conductor, which was previouſly made uſe of, ſoldered on to the end of the wire inſtead of the other point; and to this conductor ſo ſoldered I occaſionally applied the ſmall ball which was uſed in the former experiments, and which has hitherto

hitherto been called the rounded end. It was made to fit upon the point of the ſoldered conductor by means of a ſocket, ſo as to render the communication perfect. With theſe different terminations, and thus circumſtanced, all the experiments were repeated; and it was found, that the reſults were rather more in favour of the doctrine hitherto advanced, than before the communication was made ſo perfect.

It having been further objected, that the motion of the model employed in theſe experiments was conſiderably greater than the motion of a thunder cloud, I made the following experiment.

XXXIſt EXPERIMENT.

The weight which moved the model in the preceding experiment was gradually reduced till it was nearly balanced by the friction; and when the motion was rendered ſo ſlow as ſeven feet ſeven inches in ſeven ſeconds, it was very little accelerated; and in this ſtate the great cylinder being charged, the model was ſuffered to paſs: and though the velocity was leſs than three quarters of a mile in an hour, the point was ſtruck.

This experiment was repeated ſeveral times, with the ſame ſucceſs, in the preſence of ſeveral gentlemen.

Having made two other experiments, reſpecting the different terminations of conductors, and in very different circumſtances from any that have been yet related, I ſhall here give an account of them.

XXXIId EXPERIMENT.

When the great ball of the leſſer ſubſtitute was placed at the greateſt ſtriking diſtance from the ball at the end of the great cylinder, I fixed a needle into the under ſide of the remote end of the leſſer ſubſtitute, with the point downwards: oppoſite to this point, and upon the ſame ſtand, deſcribed in the XXIId experiment, was fixed another needle; ſo that the two points were appoſed to each other. The ſpace between them was varied from time to time, in order to find the greateſt diſtance at which the lower one could be ſtruck. Upon charging the great cylinder, it appeared, that the greateſt diſtance in this caſe was five inches and a quarter.

XXXIIId EXPERIMENT.

Upon repeating this experiment, whilſt every circumſtance remained the ſame, excepting that, inſtead of the point below, a rounded end was put in its place, and after charging the cylinder again, it appeared, that the greateſt diſtance at which the lightning (from the needle) ſtruck the rounded end, was not more than two inches and three quarters. And the largeneſs of the ſpark, as likewiſe the loudneſs of the exploſion, appeared to be leſs conſiderable than in the XXXIId experiment.

The reaſon for placing the needle, which was fixed at the end of the leſſer ſubſtitute, with the point downward, was to repreſent a fragment, or jagged part of a cloud, which ſometimes hangs down towards the earth; and, as Dr. Franklin and others have ſuppoſed, ſerves as a kind of ſtepping-ſtone for the lightning to paſs more readily, and in a *ſilent* manner, from a charged cloud to a pointed conductor underneath it. But we ſee, from the two laſt experiments, the lightning does not paſs in a *ſilent* manner, becauſe the

point below, as well as the rounded end, was always ſtruck, and the former at twice the diſtance nearly to that of the latter.

That the two points appoſed to each other, in the manner deſcribed above, ſhould ever occaſion a ſtroke of lightning, may perhaps appear ſtrange to thoſe who are not very well acquainted with this ſubject. However, I have related it not only as it is a fact of a very curious kind, but as the conſequences which may be drawn from it ſeem to be conſiderable.

During the courſe of this inquiry, having occaſion to try ſome experiments in the dark, I obſerved a curious circumſtance, which ſeemed to ſhew, that a point had a far greater influence upon the charged ſubſtitute, in certain circumſtances, than a rounded end had when it was placed in the ſame ſituation.

XXXIVth EXPERIMENT.

The circumſtance alluded to was an appearance of light upon the braſs ball (for ſo I call it, to be more clear in the deſcription) that was fixed

fixed at the end of the great cylinder, when the copper ball (for ſo I call *that* for the ſame reaſon) of the leſſer ſubſtitute was appoſed to it at the greateſt ſtriking diſtance, (as in the XVIIIth experiment) every other circumſtance remaining the ſame: and whilſt the model, with its pointed conductor, ſtood upon the table directly under the tin ball, that was fixed at the remote end of the leſſer ſubſtitute. For ſoon after ſeven or eight turns of the wheel, a light began to appear on the braſs ball, and continued to increaſe in brightneſs till the moment it burſt forth in an exploſion towards the copper ball. The part of the braſs on which the light appeared was that next to the copper ball; and the general appearance of it was round, and ſometimes more than half an inch in diameter. It did not ſend forth rays or ſtreams that were luminous; neither did it extend beyond the ſurface; or the diſtance to which it did extend was ſo inconſiderable, as to ſeem incapable of being aſcertained, even at the inſtant before the exploſion, when it was moſt vivid; though at that time there did appear ſomething like a ſmall ſwell towards the center, as if

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it was making an effort to get out. Whenever the wheel was ſtopped ſuddenly, or the motion of it decreaſed, the light retired on the inſtant, and totally vaniſhed; but when the motion of the wheel was renewed for a little time, the light returned as before. The whole time that this appearance continued, was never more than five or ſix ſeconds: reckoning from the moment it was firſt ſeen, to the inſtant when the exploſion happened. The diſtance between the point and the tin ball meaſured three inches and a quarter. There was no ſuch appearance on the copper ball; nor is it eaſy to conceive how there ever ſhould, when all the circumſtances are taken into conſideration.

XXXVth EXPERIMENT.

Upon repeating this experiment with a rounded end, inſtead of a point, and at the ſame diſtance from the tin ball, notwithſtanding every other circumſtance continued the ſame, there was no ſuch appearance.

XXXVIth EXPERIMENT.

But when the rounded end was moved considerably nearer, that is, within ſix tenths of an inch, a light was viſible; but then it was faint, and not more than one tenth of an inch in diameter, even at the inſtant before the exploſion happened.

Thirteenth obſervation. By the firſt of theſe experiments it appears, that the influence which the point had upon the whole of the fluid contained in the great cylinder, was ſuch as to cauſe a general tendency of it towards the leſſer ſubſtitute: but, on account of the reſiſtance which ſeemed to operate at the ſurface of the braſs ball, it was there ſtopped, and by degrees accumulated, till ſuch time as the accumulation was great enough to overcome that reſiſtance. Now, according to this manner of reaſoning, the point did not draw the fluid out of the great cylinder *ſilently*; but when the accumulation had got to a ſufficient degree, a ſudden exploſion enſued, more or leſs violent, according to the circumſtances which accompanied the experiment.

Fourteenth

Fourteenth obſervation. From the other experiment it appears, that the rounded end had not ſo great an influence as the point upon the charge in the cylinder; becauſe we were obliged to bring it five times nearer before any light could be perceived at all; and even then it was ſo faint and inconſiderable in its diameter, (rather leſs than one tenth of an inch) compared with the other light produced by the influence of the point, that it manifeſtly confirmed the truth of the laſt obſervation.

Theſe facts being in my opinion ſo clear and ſatisfactory, in regard to the great object we have had in view, I think that any further experiments reſpecting the nature and uſe of conductors are unneceſſary. I ſhall therefore proceed to make ſome general deductions from what has been already related.

It ſeems to be clear, that in all experiments made with pointed and rounded conductors (provided the circumſtances be the ſame in both) the rounded ones are by far the ſafer of the two, whether the lightning proceeds from one or more clouds; that thoſe are ſtill more ſafe, which (inſtead

ſtead of being, as Dr. Franklin recommends, ten feet high) are very little, if at all, above the higheſt part of the building itſelf; and that this ſafety ariſes from the greater reſiſtance exerted at the larger ſurface.

The luminous appearance at the end of the braſs ball, occaſioned by the point in the XXXIVth experiment, manifeſtly ſhewed that there was an accumulation of the fluid within that part of the ball, in conſequence of ſome reſiſtance; for when the reſiſtance at the ſurface of the braſs ball was at laſt overcome, by the influence the point had upon the charge, the exploſion took place immediately; and that, not only between the two ſubſtitutes, but alſo between the end of the leſſer ſubſtitute and the point.

A cloud therefore that happens to be charged, and within the ſtriking diſtance of another cloud which is not charged, and alſo equally within the influence of a pointed conductor, muſt neceſſarily produce ſimilar effects with thoſe mentioned in the XXXIVth experiment.

On the other hand, clouds that are circumſtanced like thoſe above, and not within the in-

fluence of a rounded conductor, will paſs quietly over ſuch a termination, and without any exploſion.

Nor can any one at all acquainted with ſubjects of this kind, want to be reminded, how far the effects of experiments, made within the limited power of ſuch an apparatus, muſt differ in degree from thoſe which are exhibited in the great and wonderful phœnomena of nature.

If I could grant to thoſe who object to the motion employed in ſome of theſe experiments, that a cloud charged with lightning, and motionleſs, may impend over a building; it muſt nevertheleſs be allowed, that clouds are generally obſerved to move with conſiderable velocity; ſo that when the velocity is from three quarters of a mile, or ſomething leſs, to four or five miles or more in an hour, the pointed conductor is always ſtruck by the charge contained in the great cylinder. And the experiments, which have been made by the addition of the leſſer ſubſtitute, ſhew alſo, that no ſecurity can be expected from a pointed conductor when a thunder cloud is even entirely at reſt.

Upon ACCELERATION *and its effects.*

FROM confidering the extraordinary effects which have fometimes been produced upon grofs matter by lightning, and the diftance there frequently is between thunder clouds and the earth, when fuch effects take place, I fufpected that thofe effects might, in fome degree, be owing to an increafe of the velocity of the fluid which produced them.

To try whether this was really fo, it feemed neceffary to have an apparatus of a far greater length than the great cylinder. I therefore made ufe of the long wire, occafionally, which has been already defcribed.

XXXVIIth EXPERIMENT.

Upon connecting one end of this long wire with the further end of the great cylinder, and the other, with one end of the brafs drums, I found, that about fix uniform turns of the wheel, with a moderate velocity, were required to caufe the appearance of a fmall ftream of light at the top of the fpout defcribed in the firft experi-

 ment,

ment, when the model, with the pointed conductor upon it, ſtood directly under the great cylinder, but at the diſtance of five inches.

XXXVIIIth EXPERIMENT.

When the great cylinder was unconnected with the long wire and braſs drums, and whilſt the model with the ſame conductor upon it remained in its place, about two turns, with the ſame velocity, were ſufficient to charge the great cylinder, ſo as to cauſe a ſimilar appearance in the ſpout.

XXXIXth EXPERIMENT.

On ſeparating the great cylinder from a fourteenth part of it, (which fourteenth part for the preſent I call the little cylinder) the model and machine continuing in their places, it was found, that half a turn of the wheel was ſufficient to charge the little cylinder, ſo as to cauſe the like appearance at the ſpout.

Fifteenth obſervation. Now theſe differences in the number of turns required for cauſing ſimilar appearances, when the ſeveral charges were given

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in the XXXVIIth and XXXVIIIth experiments, could not ariſe from a difference in the quantity of metallic matter contained in the reſpective ſubſtitutes; becauſe the tin-foil which covered the great cylinder (independent of the nails and wood it contained) was found to be near three times heavier than the weight of the whole wire.

Neither could theſe differences be owing to a difference in the quantity of ſurface of the reſpective ſubſtitutes; becauſe the ſurface of the great cylinder was found to be ten times greater than the ſurface of the wire.

Thoſe ſeveral differences muſt therefore depend upon ſome other cauſe; and, as a true knowledge of this cauſe may be of ſome moment in the preſent enquiry, we muſt endeavour to find it out by experiments and obſervations.

To this end, it may be neceſſary, that our enquiry ſhould begin from an early part of this ſubject, ſo that we may proceed regularly, ſtep by ſtep, as nature directs. By purſuing this method, though it is propoſed to be done very generally, (on account of the length of this paper)

per) we may poſſibly arrive at the knowledge of the cauſe in queſtion, and ſhorten the road to the main object in view, I mean, the effects of comparative velocities.

From the nature of this ſubtile and elaſtic fluid, and its being diffuſed throughout the whole earth, as well as the air ſurrounding it, the leaſt violence exerted muſt neceſſarily diſturb it.

And although experience hath taught us, how to vary the natural quantity of this fluid in many ſubſtances, in conſequence of violence; yet the ſame experience hath likewiſe taught us, that we cannot increaſe that quantity in any particular ſubſtance, without taking it from the general ſtock contained in the earth or air ſurrounding it. And therefore, when this fluid is ſo increaſed, it may be properly ſaid to be in an unnatural ſtate; and whilſt it remains ſo, muſt (from its elaſtic principle) be always endeavouring to recover its natural one.

But experience hath alſo taught us, that metal, for example, hath a property of receiving this fluid more readily, whenever it is diſturbed, than moſt other ſubſtances. For which reaſon a notion

a notion hath prevailed with many, that this property of metal arifes from a power of *attraction*, which they fuppofe it poffeffes in a greater degree than any other fubftance.

If this philofophy were true, it would follow, that the fame power, which attracted the fluid into metal, ought to keep it there. For it cannot be fuppofed to attract the fluid at one time, and then let it go at another; this would be abfurd, and contrary to experience. We muft therefore try to find a better reafon why metal is poffeffed with the property of receiving this fluid more readily than other fubftances.

XLth EXPERIMENT.

When the great cylinder with the wire and brafs drums were charged with a very fmall quantity of this fluid, by the wheels being turned fomething lefs than a quarter round, there was, the moment after, a vifible explofion and a fenfible effect perceived at the remote end of the wire. When half a turn was given, thefe effects were greater; and, after a whole turn, the

quantity of the fluid accumulated in this great apparatus was increaſed conſiderably.

Sixteenth obſervation. Now, ſomething muſt have hindered the fluid from getting out of the cylinder and wire, all the time they were charging, otherwiſe we ſhould not have been able to have cauſed the leaſt accumulation; for, from the nature of this fluid, there cannot be any accumulation, without ſome reſiſtance to occaſion it. And whatever the nature of that reſiſtance may be, experiments ſhew, that there are certain bounds preſcribed to its power of acting, and which in particular circumſtances ſeem to be very eaſily ſurmounted.

From Sir Iſaac Newton's obſervations *, and a great variety of experiments made ſince his time, we collect, that this principle of reſiſtance is probably exerted at, or very near, the ſurface of bodies, and extends only to very ſmall diſtances from them.

It ſeems then, that it is by this kind of reſiſtance at the ſurface of bodies, that the fluid is prevented from eſcaping out of the great cylin-

* Newton's Optics, pages 240, 241, and 372.

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der and wire, whilſt it is accumulating within them. And therefore, when we begin to charge the great apparatus at the nearer end, the moment any part of the charge arrives at the further end of the wire, it is prevented (in ſome degree at leaſt) from eſcaping, in conſequence of the reſiſtance it meets with at that end. And, if we continue to make the charge greater, the charge itſelf, during its increaſe, muſt alſo reſiſt every further effort which any way tends to make it greater, with a force probably proportional to the quantity accumulated.

XLIſt EXPERIMENT.

When the great cylinder and wire with the drums were fully charged, and a perſon, ſtanding upon the wire which communicated with the well, ſuddenly approached the braſs drums with his hand, an exploſion enſued, which indeed was neither ſo large, nor did it take place at ſo great a diſtance, as might have been expected;* never-

* The circumſtances attending the exploſion in this experiment were certainly owing to the long wire being not entire, but conſiſting of ſeveral pieces twiſted together, the ends of which being very many, muſt have cauſed a conſiderable part of the fluid to eſcape, and ſo have weakened the general effect.

thelefs, the perfon received a violent fenfation, not unlike that produced by the Leyden phial, as it affected his body quite through, from the hand that took the difcharge, to the feet that ftood upon the wire.

XLIId EXPERIMENT.

Upon repeating the experiment above, with the great cylinder only, and when it was fully charged, the explofion appeared ftronger, and the diftance it ftruck at greater, than in the other cafe; and yet the fenfation received was not near fo violent as when the long wire was connected with it.

XLIIId EXPERIMENT.

When the little cylinder by itfelf was fully charged, the effects were very inconfiderable compared with thofe from the great cylinder. For, in this cafe, the perfon ftanding upon the wire of communication was affected in his hand only, and that no further than the wrift. Thefe three experiments were repeated many times by different

different perſons, and the reſults were nearly the ſame.

Seventeenth obſervation. When all the circumſtances in the two laſt experiments are conſidered, we may ſafely conclude, that the difference in the ſenſation, produced by the two cylinders, could ariſe from no other cauſe than a difference in their lengths; the one being fourteen times longer than the other, and both in other reſpects nearly ſimilar; and ſince the ſenſation perceived in the XXXVIIIth experiment, where the long wire was employed, was conſiderably greater than when the great cylinder alone was charged, we ſeem to have ſufficient reaſon to apprehend that the effects of every charge, as to ſenſation, will be proportional to the length of the body charged, provided the charge (or accumulation) be uniform from end to end in every experiment.

Apprehending that if ſome of the circumſtances employed in producing the charge were varied, we might poſſibly obtain a greater charge than we had yet found, I made the following experiment:

XLIVth EXPERIMENT.

" Inſtead of one machine to charge the great
" apparatus, I made uſe of two. The glaſs cy-
" linders belonging to each were of the ſame
" length and diameter nearly. One of thoſe ma-
" chines was continued in its uſual place, which
" was not far from the nearer end of the great
" cylinder; the other ſtood at the further end
" of the braſs drums. After connecting the long
" wire with the great cylinder and braſs drums,
" in the manner before deſcribed, the wheels of
" both machines were put into motion, with
" equal and uniform velocities; and after ſix
" turns of each wheel, (for I could not prevail
" upon any one preſent at the time to take a
" higher charge) and after waiting above eight
" ſeconds, the perſon ſuddenly approached the
" braſs drums with his hand, immediately an ex-
" ploſion took place, and a diſagreeable ſenſation
" was perceived. The diſcharge was then made
" at the nearer end of the great cylinder, and
" there ſeemed to be no difference in the effect.

XLVth EXPERIMENT.

" Upon repeating this experiment with one
" machine only, and after the ſame number of
" turns of the wheel with the ſame velocity, and
" after waiting above eight ſeconds alſo, the
" ſame perſon ſuddenly cauſed an exploſion with
" the ſame hand. But the ſenſation in conſe-
" quence of it was very different from the laſt
" experiment. For he declared it was little more
" (if that) than half as violent. Theſe laſt ex-
" periments were alſo repeated ſeveral times by
" another perſon, who gave the ſame account of
" the reſults." *

XLVIth EXPERIMENT.

I now charged the long wire only and fully with one machine; the exploſion, in this caſe, appeared not very large, but of a reddiſh hue, and the diſtance it ſtruck at was not more than

* *N. B.* On reconſidering theſe two laſt experiments, I find, that inſtead of giving ſix turns to the wheel, in the laſt experiment, I ought to have given twelve, in order to make any compariſon between the effects of the two experiments: I therefore ſimply relate the experiments as they were made, without making any deductions from them.

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one inch and a half; however, the ſenſation acroſs the body was at that inſtant ſharp (as it was expreſſed) and violent, but not quite ſo diſagreeable as when the great cylinder was connected with it, and ſimilarly charged.

XLVIIth EXPERIMENT.

Having procured an equal quantity of the ſame kind of wire, and of the ſame diameter, with that which was ſuſpended and tried in the laſt experiment, it was placed in the form of coils upon a board, fixed on the top of a long ſtand of glaſs, without having any connection whatſoever with the great apparatus, or any part of it. Theſe coils were then fully charged by the power of one of thoſe machines only. The ſenſation they afforded, in conſequence of cauſing ſparks, was very inconſiderable, compared with what had been obſerved in the laſt experiment.

XLVIIIth EXPERIMENT.

The ſeveral coils of wire employed in the laſt experiment, as likewiſe ſeven hundred yards more

more in coils alſo, were joined together at their ſeveral ends. Theſe coils being then ſtrung upon ſilk lines, were drawn out into a form reſembling that of a ſcrew, and ſeparated from each other in ſuch a manner all along as to occupy one hundred yards of ſilk line. The ſeveral diameters of theſe coils, at a mean, were about fifteen inches. As I had ſo ſhort a time in which to prepare and ſuſpend them properly, the diſadvantage of their touching, and interſecting each other in many places, could not be prevented; however, I found that the ſenſation cauſed, after charging this wire, was nearly equal to that which had been experienced from the long wire in the XLVIth experiment.

XLIXth EXPERIMENT.

Upon joining the further end of theſe coils to one end of the long wire, ſo that the whole length was in this experiment about 3900 yards, and afterwards charging the nearer end of the coils, and without the great cylinder, (it being at that time taken down) the ſenſation complained of, by two indifferent perſons, was twice as violent

violent as the ſenſation perceived by the ſame perſons when the long wire alone was charged.

It may be now proper to make ſome general obſervations reſpecting the *exploſion* itſelf, and the quantity of the fluid diſcharged in conſequence thereof.

After many experiments, we found, that when the great apparatus was fully charged, and the motion of the wheel ſuddenly ſtopped, it appeared, that a ſingle exploſion at either end of it inſtantly (as to ſenſe) diſcharged the fluid contained therein; but never ſo effectually as to leave no remainder: for the quantity which did remain (upon a ſecond application immediately afterwards) was generally ſufficient to cauſe a ſecond exploſion perceptible to the ſenſe of feeling, as well as to that of ſight.

Now, before the great exploſion was cauſed, the fluid accumulated in the apparatus muſt have been diffuſed equally through it, in conſequence of its elaſtic principle; and, being ſo circumſtanced, a *ſudden* application of the hand, or any other ſubſtance, which would open a door for

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the paſſage of the fluid into the earth, was found to diſcharge the greater part of that fluid: and whatever part thereof was ſo diſcharged, the moſt diſtant particles ſeemed to have arrived at the point where the exploſion took place, at the ſame time with thoſe that were the neareſt to it; becauſe, immediately after the exploſion, there was very little of the fluid remaining in the apparatus.

If then the diſcharge of the fluid be, as it ſeems to be, very nearly inſtantaneous, the particles of it muſt move with velocities, and conſequently with forces very nearly proportional to thoſe diſtances.

From this conſideration, I apprehend, it will appear why the ſenſation upon the diſcharge from the long wire, in the XLVIth experiment, was more violent: and upon recollecting the XXXVIIth, XXXVIIIth and XXXIXth experiments, and the obſervations I made upon them, I am inclined to believe that the effect depends more upon the length of the metallic body, than upon the quantity of its matter or ſurface.

It was upon the idea of the velocity of the fluid being thus increaſed, that I apprehended gunpowder might be fired without the leaſt appearance of a ſpark. The ſucceſs of this experiment was an inducement to try Kunkell's phoſphorus, which was made by Dr. Higgins. The moment this inflammable ſubſtance was brought very near the ſurface of the braſs drums, it burſt into a blaze; and common tinder, applied in the ſame manner, was ſet on fire, the inſtant when it was brought ſo near as to touch the metal: but there was not the leaſt appearance of a ſpark in any of theſe experiments.

The method taken to fire the gunpowder was this: Upon a ſtaff of baked wood a ſtem of braſs was fixed, which terminated in an iron point at the top. This point was put into the end of a ſmall tube of Indian paper, made ſomewhat in the form of a cartridge, about one inch and a quarter long, and about two tenths of an inch in diameter. When this cartridge was filled with common gunpowder (unbruiſed) the wire of communication with the well was then faſtened to the bottom of the braſs ſtem. Being ſo circumſtanced,

cumſtanced, and whilſt the charge in the great cylinder and wire was continually kept up by the motion of the wheel, the top of the cartridge was brought ſo near to the drums as frequently to touch the metal. In this ſituation, a ſmall, faint, luminous ſtream was obſerved between the top of the cartridge and the metal drum.

Sometimes this ſtream would ſet fire to the gunpowder at the inſtant of the application; at others, it would require half a minute, or more, before it took effect. But this difference in time might probably ariſe from ſome difference in the circumſtances, for any the leaſt moiſture in the ſilk lines, the powder, or in the paper itſelf, was unfavourable to the experiment.

This new method of firing gunpowder by a luminous ſtream of the matter of lightning, ſurely merits the moſt ſerious attention; and more eſpecially in thoſe caſes where pointed conductors are fixed to ſecure magazines of gunpowder from ſuch accidents.

In repeating the laſt experiment, there were one or two inſtances where the powder was fired without making uſe of the long wire; but never

with the great cylinder alone. For we were obliged to connect it with the braſs drums by means of a wire ten or twelve feet long.

It was, however, a conſiderable time (ten minutes or more) before the experiment ſucceeded even with this laſt apparatus. This was not the caſe with tinder; for it fired pretty readily, and ſometimes with the great cylinder alone.

Lth EXPERIMENT.

Before the apparatus was taken down in the Pantheon, attempts were made ſeveral days to fire gunpowder, but without ſucceſs, except in one inſtance, which was attended with ſome difficulty. This failure ſeemed to be owing to a variety of cauſes, the chief of which appeared to be moiſture that affected the ſilk lines, and perhaps the powder itſelf. I might add alſo the duſt, which in ſo long a time had ſettled upon the lines, and rendered them in ſome degree incapable of reſiſting the paſſage of the fluid. However, when the coiled wires upon the ſilk lines were properly joined to the long wire, as in the XLIXth experiment, it was found that gunpowder

powder could be fired very readily. This is another proof of the increafe of velocity by increafing the length of the wire.

The explofion of gunpowder, in the particular manner related above, without the affiftance of the Leyden charge, and even without a fpark, is an effect which could not eafily be deduced from reafoning, upon any experiments hitherto made; for though gunpowder has been frequently fired with the Leyden charge, yet there is a difference in the two cafes, which makes it neceffary to take fome further notice of that experiment.

When the Leyden phial is charged, we find by experiment, that the charge is confined in a fmall compafs, or very much condenfed, near one furface of the glafs. It is alfo true, that the oppofite furface of the fame glafs is as much rarified, or (according to Dr. Franklin, who was the firft that obferved it) in a *minus* ftate. Now, according to this rule, the greater the furface of the glafs is, (and of a given thicknefs) the greater charge may be given; and the greater the charge, the greater muft be the effect (whenever the dif-

charge

charge is properly made) to reſtore the natural ſtate of the glaſs on the two oppoſite ſides. In order therefore to fire gun-powder with this kind of apparatus, experience hath taught us hitherto that the powder ſhould be confined, for example, in a tube or cartridge; and that this cartridge ſhould be placed in ſuch a manner as to make part of the circuit neceſſary for the diſcharge of the fluid from one ſurface of the glaſs, and carry it through the cartridge to the other ſurface. But it is alſo neceſſary, before the powder can be fired, to have part of the wire, which forms the circuit, in contact with the powder; or, to have one end of the wire, which makes part of the circuit, forced a little way into one end of the cartridge; and another end of the wire, which alſo makes part of the circuit, forced a little way into the other end of the cartridge; but ſo as that the two ends of the wire within the cartridge may be at a proper diſtance from each other; which diſtance will depend upon the ſtrength of the charge. When every thing is thus adjuſted, and the circuit properly made, the gunpowder is generally fired: I ſay

I say generally, because it sometimes happens, that the charge is not great enough to produce the effect required. For this effect does not appear to proceed from the spark, or explosion produced by the fluid (because flame of a certain density will not fire gunpowder); but it must be from the ends of the two wires, (or from one of them at least) within the cartridge, being rendered hot enough to fire the gunpowder, in consequence of the very great quantity of the fluid, and the velocity with which it passed through the wire at that moment; it being well known that wire, even of a considerable thickness, has been frequently made red-hot, and even melted, by the Leyden charge.

Dr. Lind, a gentleman who is well acquainted with this subject, favoured me with a sight of a very curious experiment, which seems to shew more clearly what has been advanced above, respecting the immediate cause of the gunpowder's taking fire in the Leyden experiment. He procured for the purpose some extraordinary fine threads or shavings of steel; one of which was so disposed, where gunpowder was properly

lodged

lodged and confined, as to be in contact with the powder. After having charged a ſmall phial, (which did not exceed a pint in meaſure) with only a few turns of the wheel, he made the diſcharge, and fired the gunpowder. The advantage he gained by having ſo ſmall a thread of metal in this experiment was, that it could be made red-hot with a leſs charge than what is neceſſary when a thicker wire is made uſe of; and the ſmallneſs of the charge he employed to fire the powder was a certain proof that the ſteel thread had been made red-hot.

Upon the whole, we find, that this method of firing gunpowder is totally different from the other, where the great apparatus was employed; becauſe the faint luminous ſtream, obſerved at the braſs drums where the gunpowder was applied, I found was abſolutely incapable of making the point of metal within the cartridge red-hot. Beſides, there did not appear to be any exploſion whatſoever between the apparatus and the powder at the inſtant it was fired.

Before I conclude this paper, it may not be amiſs if I take notice of certain atmoſpheres, which

which bodies have round them when they are properly charged with this elaſtic fluid; becauſe the nature of them may not perhaps be perfectly underſtood by every one who attends to enquiries of this kind.

Every charge, from the nature of the fluid which produces it, muſt, while ſuch charge remains, continually act upon the air ſurrounding it, and of conſequence upon the fluid alſo which ſtands diffuſed therein and in the intervals between its parts, by its repulſive principle. This appears to be true, not only from theory, but experiment.

The ſtate of the fluid in the air, neareſt to the cylinder that is charged, muſt therefore be in an oppoſite ſtate to that which is in the cylinder; that is, in the one caſe it will be condenſed, and in the other rarefied.

Now becauſe the power ariſing from the charge within the cylinder, which cauſed the rarefaction beyond its outward ſurface, is limited, the diſtance to which that rarefaction extends muſt be limited alſo; and beyond that diſtance the fluid muſt be condenſed more or leſs, according to

the power of the charge which caufed the rarefaction.

Whenever therefore we are difpofed to open a door to let the charge out of the cylinder, the rarefied fluid near the outfide of the cylinder muft neceffarily promote the difcharge at that inftant.

As to the diftance to which this rarefaction extends, it will always depend upon the ftrength of the charge, and the ftate of the air in the place where the experiment is made.

When I fufpended the 1600 yards of wire in the Pantheon, the feveral lengths thereof were purpofely hung (as hath been obferved before) at five or fix feet diftance from each other, intirely upon this idea; left the refpective atmofpheres might interfere with each other, or with the charge contained in the wire, and by that means difturb the experiment.

It is now time to put an end to this enquiry; in which, however defective I may have been in ability, neither attention nor impartiality have been wanting; and although it may be true, that even before thefe experiments were tried I was inclined

inclined more to one ſide of the queſtion than the other, it was becauſe I many years ago grounded my perſuaſions upon the philoſophy of Sir Iſaac Newton. Without thoſe perſuaſions, and the advantage of that moſt gracious encouragement, which is never wanting to honeſt and candid endeavours in the purſuit of philoſophical truth, I ſhould hardly have felt ſufficient zeal to engage in ſo conſiderable an undertaking: nor, indeed, without powerful aſſiſtance, could the great object of this enquiry have been in any degree attained. For my own part, I can boaſt of very little more in this inveſtigation than patience and induſtry.

I ſhall only add, that if any one, who may be diſpoſed to try the preceding experiments, will uſe an apparatus of ſufficient dimenſions, and faithfully attend to every circumſtance as I have done, I have no doubt they will find the ſame reſults; but they muſt alſo apply their obſervation to much ſmaller circumſtances than have been here ſpecified, as the intervention of a ſingle hair, a fibre of down, or even a little vapour ariſing from perſpiration or otherwiſe, will, where

great exactneſs is required, ſometimes prevent the ſucceſs of an experiment; and by that means miſlead the obſerver, or afford a ſubterfuge for a miſtaken hypotheſis.

Great Ruſſell Street, Bloomſbury,
November 12, 1777.

N. B. The different diſtances, at which the point and ball were ſtruck, being expreſſed in fractions, (page 43) I thought it would be better to put thoſe diſtances in whole numbers, that the ratio between them may more clearly appear:

Sharp point.		*Rounded end.*
90	- - - - - - -	25
160	- - - - - - -	20
130	- - - - - - -	35
220	- - - - - - -	16
260	- - - - - - -	20
80	- - - - - - -	4

Forty of thoſe parts are equal to one inch.

MEASUREMENTS, &c. *of the great Apparatus and Machinery.*

	Feet.	*Inches.*	*lb.*
LENGTH of wire suspended, which consisted of many pieces that were connected by twisting the several ends together - - - - -	4800		
Weight of the wire - - - - - - - -			30
21 pieces of this wire, laid parallel and close to each other, measured exactly - - - - -		1	
Surface of the whole wire in square feet - -	57		
Length of coiled wire, which was suspended also, but in a form somewhat resembliug a screw, and of the same thickness with the wire above - -	6900		
Length of great cylinder, including the brass drums above - - - - - - -	155		
Diameter of this cylinder - - - - -	1	4	
Weight of tin-foil, which covered 112 drums - - -			87
Surface of 112 drums in square feet (including the six ends) measured - - - - -	592		
Length of frame on which the model moved -	18	2	
Space through which the conductor upon the model passed from its beginning to move, to the instant nearly of its being struck - - - - -	7	7	
Heighth of frame from the floor - - -	3	$1\frac{1}{2}$	
Breadth of frame - - - - - - -	1	3	
Heighth of the two posts, on the top of which were fixed two wheels - - - - -	10	6	
Diameter of the great wheel - - - -	1	11	
Diameter of the lesser wheel, fixed upon the same axis - - - - - - - - -	-	$11\frac{3}{4}$	

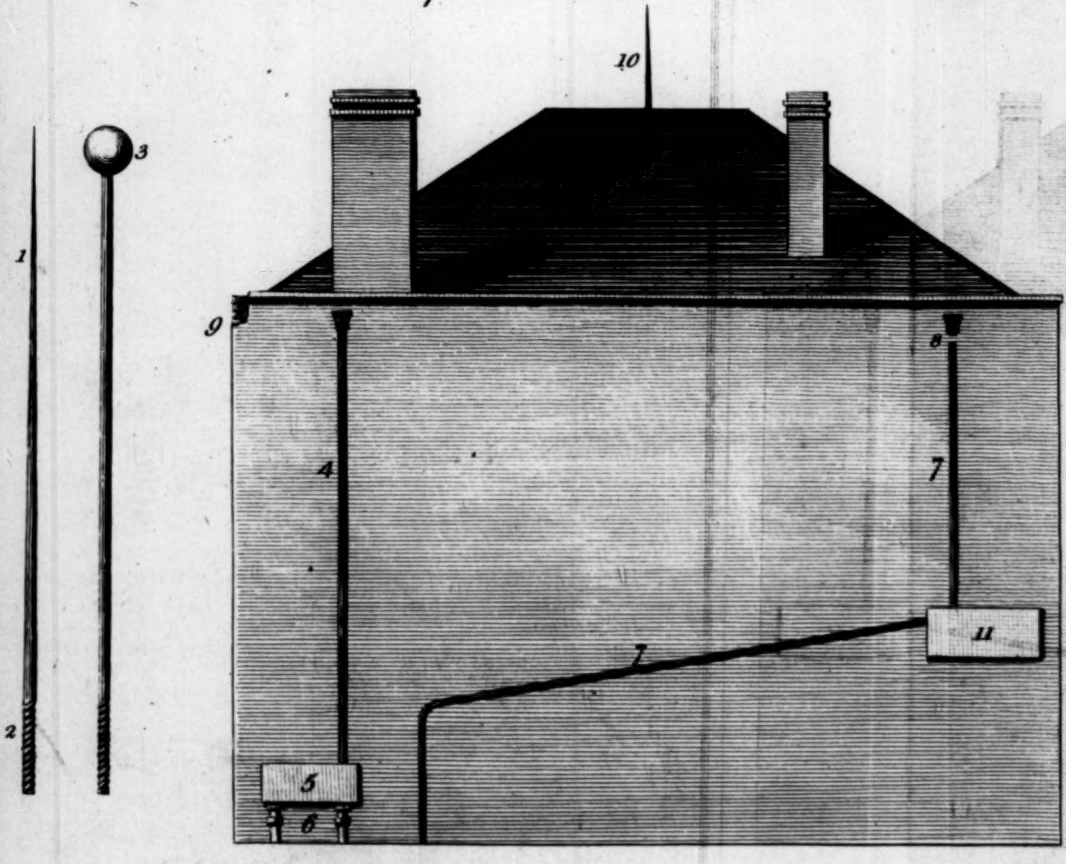
Diameter

	Feet.	Inches.	lb.
Diameter of the pulley that was fixed between the two posts, which delivered the line from the model to the greater wheel, the center of which pulley was 2 inches and ¼ above the center of resistance in the model - - - - - -		5	
Greater weight to draw the model - - - - - -			32
Lesser weight - - - - - - - - -			4
Weight of the model - - - - - - - -			12

Upon a scale of ⅓ of an inch to a foot.

	Feet.	Inches.	lb.
Length of the model - - - - -	1	9 ¼	
Breadth - - - - - - -	1	4 ⅙	
Heighth to the top of the parapet - -	1	1 11/12	
Roof above the parapet - - - - -	-	4 ⅔	
Chimnies exceeded the highest part of the roof about - - - - - - - -	-	⅙	
Pointed conductor when fixed upon the roof	-	3 ⅓	
Length of short spout - - - - -	-	10	
Length of long spout in its bent state -	1	10 ¾	
The two posts or pillars to support the cistern, (which cistern was lined with lead) belonging to the short spout - - - - -	-	⅝	

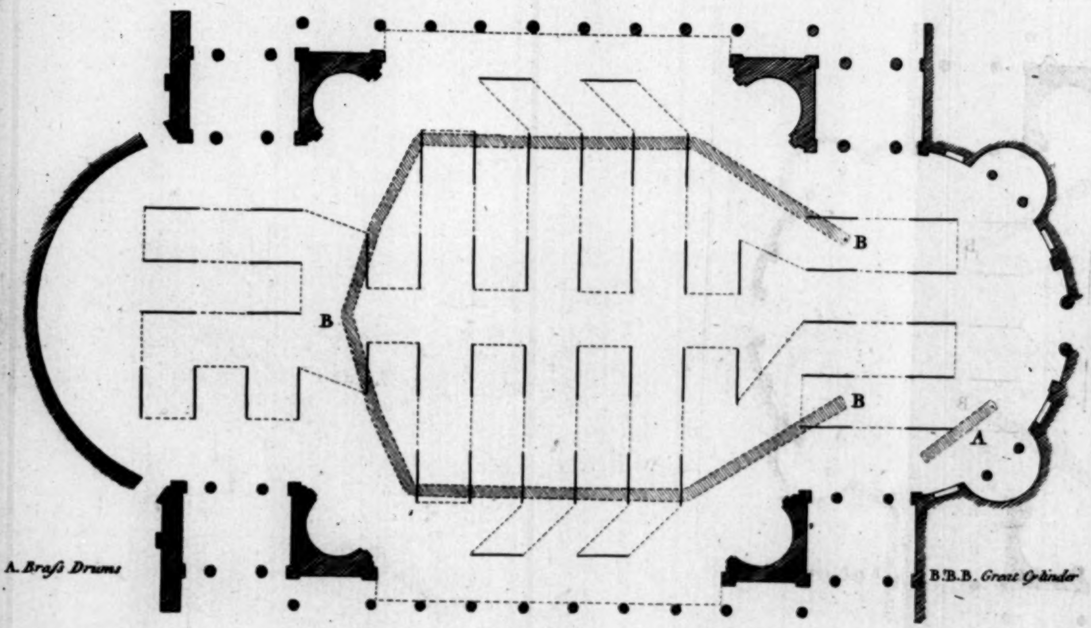
The North Elevation of the House in which the Board of Ordnance meet call'd, for that reason, the Board House.



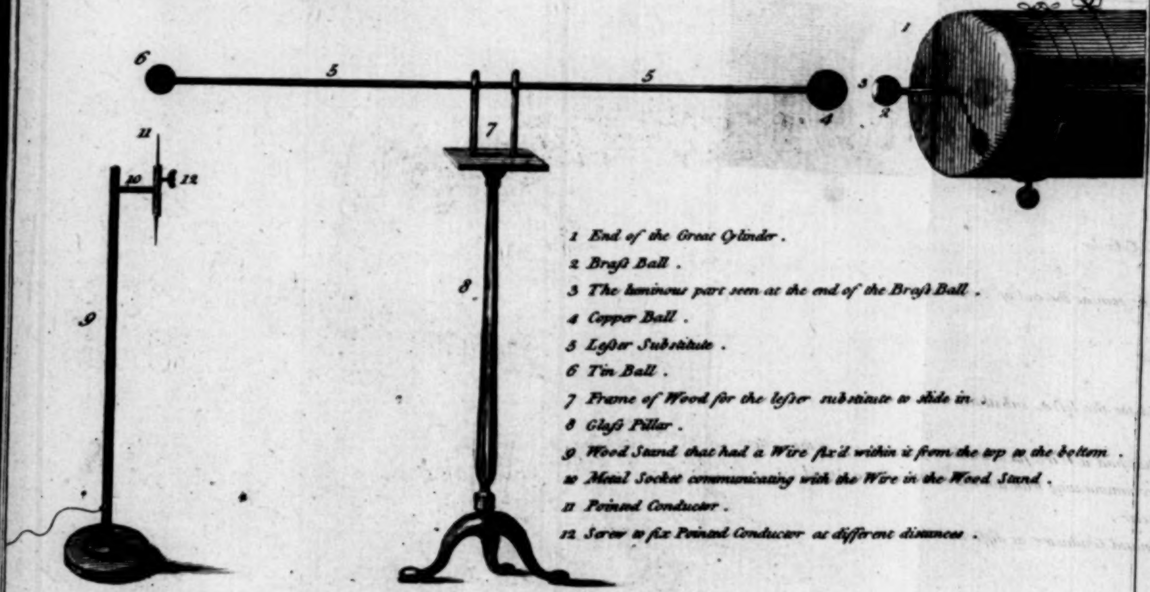
1. Size of pointed Conductor, as on the Model.
2. The part inserted at the top of the Roof.
3. Size of rounded end, as on the Model.
4. Short Spout.
5. Cestern.
6. Two Posts.
7. Long Spout.
8. Little discontinuation of Metal not in the House but left in the Model for the purpose of making Experiments.
9. The Corner damag'd by Lightning.
10. Pointed Conductor. 11. Cestern.

Basire Sc.

Disposition of the Long Wire, Braſs Drums, and Great Cylinder, as suspended in the Pantheon.



The dotted lines represent the lower, the black lines the upper horizontal parts of the Long Wire.
The perpendicular parts could not be represented in this plate and must therefore be supplied by the Readers Imagination.



Apparatus employed in the 34th experiment.

Basire sculp

NEW EXPERIMENTS

UPON THE

LEYDEN PHIAL,

Reſpecting the TERMINATION of

CONDUCTORS.

IN the LXIVth volume of the Philoſophical Tranſactions, there is a paper of Mr. Henley's, upon the ſubject of Conductors, wherein are contained ſeveral experiments, intended to ſhew that pointed terminations are preferable to ſpherical ones, for ſecuring buildings, &c. from accidents by lightning.

Upon thoſe experiments I made ſome obſervations, and particularly upon the fifth, where a point and ball were placed at the ſame diſtance from a ſphere of copper, ſo as to make part of the

the circuit in the Leyden experiment.* In the defcription of that experiment I objected to the two chains employed therein, becaufe the metallic communication was by that method confiderably interrupted, on account of a want of contact between the feveral links compofing the chains. I did not then repeat the experiment, becaufe the particular circumftances attending the Leyden phial appeared, in my judgment, very unlike what happens in nature: and therefore I contented myfelf with pointing out the feveral circumftances in which they differed; and in obferving, that according to Mr. Henley's account, the point did not protect the rounded end from being ftruck, which it ought to have done, if Dr. Franklin's philofophy was well founded.

Since that time an occafion has offered, which made it neceffary to try this particular experiment. The occafion alluded to arofe from a late inveftigation of Mr. Nairne's experiments, by Dr. Mufgrave, who was defirous of having that

* Further obfervations upon lightning by Benjamin Wilfon, publifhed in 1774.

experiment repeated, becauſe (as it ſtood in Mr. Henley's account) it ſeemed to contradict a conſiderable part of the Doctor's reaſoning.

Not being furniſhed with an apparatus to make the experiment, I requeſted the favour of Mr. Cavallo to aſſiſt me with his: and though it was not ſo compleat for the purpoſe as could be wiſhed, yet it anſwered ſufficiently well to ſhew, that an attention to the circumſtance of a perfect communication in this experiment was very material to diſcover the truth; and that the want of it, had, probably, occaſioned the ball's being ſtruck in preference to the point, as related by Mr. Henley. For upon employing a wire inſtead of the chains, the *point* was ſtruck at more than *three times* the diſtance of the ball.

Seeing ſo great a difference between the two experiments, I ordered ſuch an apparatus to be made, as I thought would be the leaſt exceptionable for the purpoſes of determining the fact upon which thoſe different appearances ſeemed to depend; namely, a perfect and an imperfect circuit of communication with the Leyden phial.

In the contriving of this inſtrument, it appeared material, in order to have it anſwer the ſame end as Mr. Lane's electrometer, that the ſeveral experiments to be tried with it, might be compared with each other, in a more accurate manner.

The circuit of communication was divided into two parts:

A bent rod of braſs, with a ball of the ſame metal, three quarters of an inch in diameter, ſcrewed on to the upper extremity of it, and a copper ball, five inches in diameter, ſcrewed on to the lower end, formed one of the parts. This part was ſupported by a ſtand of wood that had a cap of braſs at the top, into which the braſs rod was occaſionally ſcrewed.

The other part of the circuit conſiſted of a braſs rod alſo, one end of which branched out in the form of a fork, with two prongs that pointed towards the center of the copper ball; and thoſe prongs were ſo conſtructed, that either of them could be made longer or ſhorter, juſt as the experiment required. On the end of one of the prongs was fixed a ball of braſs, three quarters of

of an inch in diameter, and on the other a ſharp ſteel point or needle. The ſhoulder of this fork ſcrewed into a ſmall plate of iron, that was fixed on the inſide of a wooden veſſel, which contained the greateſt part of a cylindrical glaſs jar, twelve inches three quarters high, and about four inches in diameter. This glaſs was rather thick than otherwiſe, and the coating of it (which was tin-foil) meaſured nearly 144 ſquare inches on each ſurface. Beſides this coating, part of the inſide of the wooden veſſel was coated alſo with tin-foil, for the purpoſe of making a ſecure communication between the iron plate, and the outward coating of the jar. Within the jar itſelf was fitted a cylinder of wood, that was covered with tin-foil alſo, to make a communication between the inſide coating of the glaſs and a braſs rod, that was fixed upright in the center of the wooden cylinder. This upright rod having a ball of braſs at the end, three quarters of an inch in diameter, was bent towards the firſt part of the circuit: ſo that the two balls A and B in figure 2. being upon a level, looked towards each other, but were placed from time to time at dif-

ferent diſtances, as occaſion required; and thus anſwered the purpoſe of an electrometer.

Whilſt this inſtrument was making, Dr. Lind propoſed to Mr. Cavallo and myſelf, that we ſhould ſee ſome experiments at his houſe, which he had made in conſequcnce of thoſe we had before tried at Mr. Cavallo's.

The apparatus he made uſe of was but ſmall, (ſee fig. 1.) for the phial contained very little more than a pint; and the coating on its outward ſurface meaſured no more than 39 ſquare inches.

The reſults of the ſeveral experiments we made, are contained in the firſt of the following tables: from which it will appear, that in 23 experiments, there was not any *one inſtance* where the ball was ſtruck at a greater diſtance than the point, nor even at the ſame diſtance. It is remarkable, that in two or three experiments, where the point was farther off than the ball, both the point and the ball were ſtruck at the ſame time; which ſhews, that the influence of the point, although placed at a greater diſtance, was

was equal to the influence of the ſpherical termination placed conſiderably nearer.

When the forked inſtrument and electrometer were finiſhed, it was found, that a ſuperior force was neceſſary to charge the jar belonging to it, (on account of its thickneſs) than what we had employed in our firſt trials.

Upon an application to Dr. Higgins, he favoured me with the uſe of his machine; the cylinder of which, when excited with the aſſiſtance of his amalgama, acted ſo powerfully, that it charged the jar, accompanying the new inſtrument, very readily.

We began the experiments where the electrometer was ſtruck at the greateſt diſtance, and then adjuſted the diſtances of the ball and point from the copper ball accordingly; ſo that if the point was ſtruck, when they were adjuſted, the moving of the ball the thirty-ſecond part of an inch would occaſion the ball to be ſtruck in preference to the point, and *vice verſa.* Afterwards we leſſened the ſtriking diſtance of the electrometer, in every experiment, till we attained the leaſt diſtance.

Upon

Upon reverſing part of the apparatus, as in fig. 3. all thoſe experiments were repeated again; the copper ball being put neareſt to the glaſs, in the place of the forked part, and the forked part in the place of the copper ball. This ſet of experiments being compleated, we made others, where the ball only was oppoſed; and after them, where the point only was oppoſed to the copper ball.

Having gone through all theſe experiments, as they are ſet down in the ſecond table, we then repeated the experiment with the chain, after Mr. Henley's manner. The reſult of which, and with the apparatus reverſed, will appear in the third table.

The chain we employed upon this occaſion was of iron, and *very ruſty*; no other being then at hand.

To avoid every objection, it was reſolved upon, that all the experiments we had made at Dr. Higgins's, ſhould be repeated; but with the two chains, inſtead of the forked apparatus.

On the 23d of *June*, by the favour of Mr. Partington, (Dr. Higgins having diſpoſed of his

machine

machine at that time) we went through the whole of the experiments, thus circumſtanced. The chains employed were braſs, and a glaſs ſtand ſupported the ball and point. Mr. Partington's cylinder meaſured about thirteen inches in diameter. This glaſs, with the aſſiſtance of Dr. Higgins's amalgama, acted powerfully. All theſe experiments are contained in the fourth table.

Before this paper is concluded, it is neceſſary to caution thoſe who may be diſpoſed to repeat the experiments mentioned in the ſeveral preceding tables, that a ſtrict attention be had to every the leaſt circumſtance, relative to the making of the experiment; it being ſo difficult to preſerve the intended diſtances between the reſpective parts of an apparatus, not perfectly executed, (as is frequently the caſe with all new inſtruments) that we could not ſucceed in adjuſting the diſtances of the electrometer, ſo as to be exactly in an arithmetical progreſſion.

B. WILSON.

June 23, 1778.

TABLE I.

EXPERIMENTS made at Dr. LIND's, *June* 18, 1778, with the LEYDEN PHIAL.

		Point and Ball oppoſite the Leyden Phial.		*Ball only.*	*Point only.*	*Apparatus reverſed.*		*Ball only.*	*Point only.*
I.	Electrometer	68	— — —	65	65	64			
	Ball	18	both ſtruck twice at the ſame time.	59	—	18	both ſtruck at the ſame time.		
	Point	24		—	112	24			
II.	E.	64	— — —	54	54	— —		54	54
	B.	18	both ſtruck twice at the ſame time.	56	—	— —		51	—
	P.	24		—	95	— —		—	66
III.	E.	40	— — —	—	—	40.			
	B.	18	— — —	—	—	18.			
	P.	25	— — —	—	—	24.			
IV.*	E.	28	— — —	28	28	— —		28	28
	B.	34	ſtruck alternately.	26	—	— —		23	—
	P.	73		—	68	— —		—	44
V.	E.	24.							
	B.	44.							
	P.	60.							
VI.	E.	22	— — —	—	—	22.			
	B.	18	— — —	—	—	18.			
	P.	46	— — —	—	—	26.			
VII.	E.	18	— — —	—	—	18.			
	B.	18	— — —	—	—	18.			
	P.	48	— — —	—	—	36.			
VIII.	E.	16	— — —	—	—	16.			
	B.	20	— — —	—	—	15.			
	P.	56	— — —	—	—	31.			

N. B. Eighty of thoſe parts make one inch.

The number oppoſite the word electrometer, denotes the diſtance between the balls which conſtituted the electrometer; and the numbers oppoſite to the words *ball* and *point*, ſhew the greateſt diſtance at which they were reſpectively ſtruck.

* The point and ball, in this experiment, were not directed immediately towards the outſide coating of the jar, but towards the broad ſurface of a common tea-caniſter; the oppoſite outward ſide of which was in contact with the coating of the jar.

JAMES LIND.
TIBERIUS CAVALLO.
BENJAMIN WILSON.

TABLE II.

EXPERIMENTS made at Dr. HIGGINS's, *June* 19, 1778, with the LEYDEN PHIAL and *forked* Apparatus.

N. B. The measures expressed in the following tables, were taken from a scale, containing 32 parts in one inch.

		Ball and Point opposite the Leyden Phial.	*Ball only.*	*Point only.*	*Apparatus reversed.*					*Ball only.*	*Point only.*
I.	Electrometer	32	32	32	32	—	—	—	—	32	32
	Ball	34	48	—	34	—	—	—	—	36	—
	Point	45	—	88	42	—	—	—	—	—	42
II.	E.	28	28	28	28	—	—	—	—	28	28
	B.	30	43	—	36	—	—	—	—	33	—
	P.	38	—	78	42	—	—	—	—	—	39
III.	E.	25	26	26	25	—	—	—	—	26	26
	B.	28	36	—	31	—	—	—	—	32	—
	P.	37	—	67	32	—	—	—	—	—	33
IV.	E.	20	20	20	20	This experiment being repeated at intervals, other experiments intervening, gave the following results.		20	20	20	20
	B.	28	29	—	29			28	30	25	—
	P.	51	—	64	28			27	29	—	24
V.	E.	16	16	16	16	—	—	—	—	16	16
	B.	22	20	—	22	—	—	—	—	23	—
	P.	44	—	47	24	—	—	—	—	—	26
VI.	E.	13	13	13	13	—	—	—	—	13	13
	B.	21	14	—	16	—	—	—	—	18	—
	P.	38	—	36	22	—	—	—	—	—	22
VII.	E.	10	10	10	10	—	—	—	—	10	10
	B.	12	10	—	13	—	—	—	—	12	—
	P.	18	—	25	20	—	—	—	—	—	20

TABLE III.

EXPERIMENTS with the CHAIN, after Mr. HENLEY's manner.

	Point and Ball opposite the Leyden Phial.				*Apparatus reversed.*		
Electrometer	21	—	—	—	23	repeated at different times.	23
Ball	26	—	—	—	28		26
Point	24	—	—	—	26		30

JAMES LIND.
TIBERIUS CAVALLO.
BENJAMIN WILSON.

TABLE IV.

The EXPERIMENTS of the 2d and 3d Table, repeated at Mr. PARTINGTON's, *June* 23, 1778, a Braſs CHAIN being made uſe of inſtead of the *forked* Apparatus.

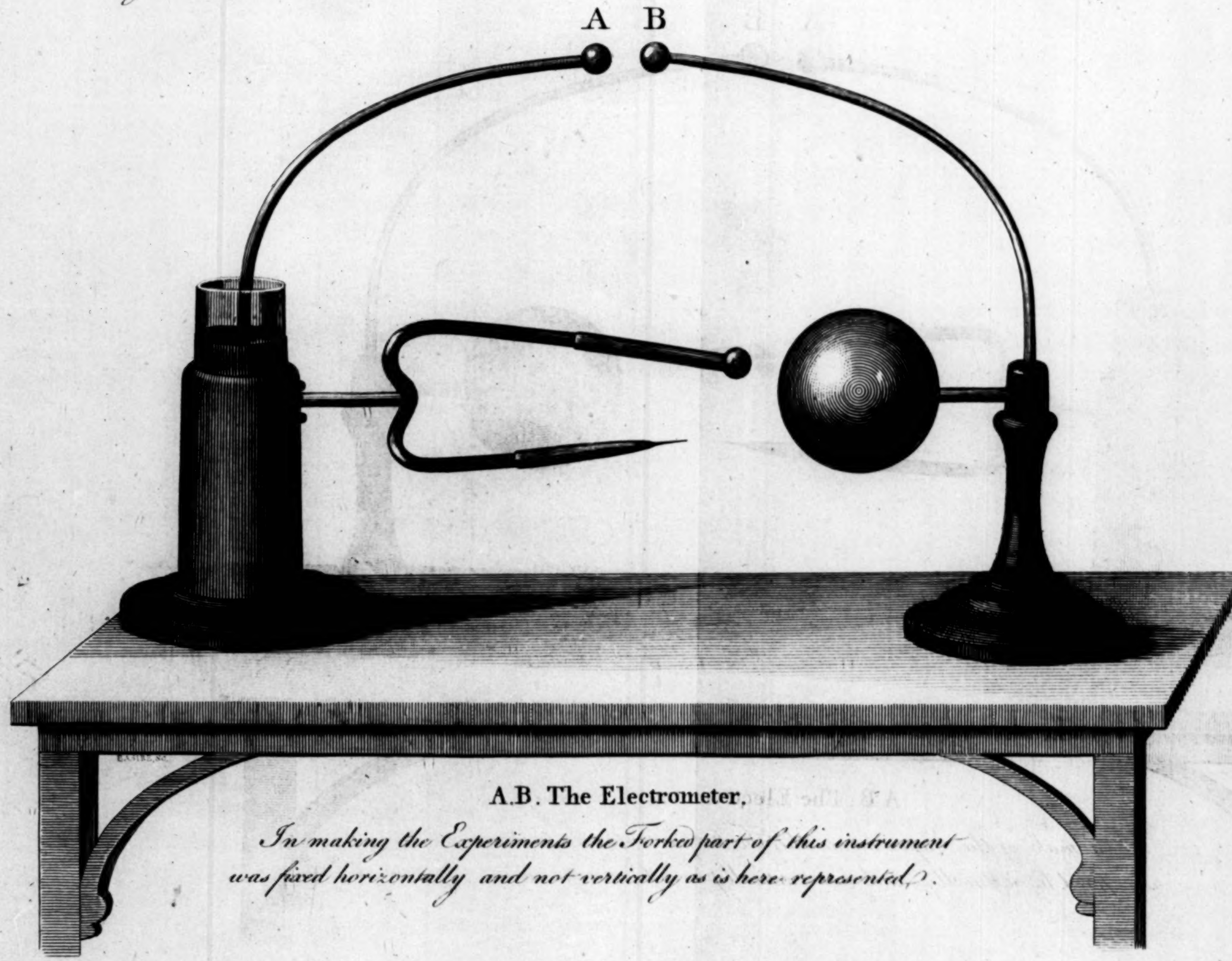
		Ball and Point oppoſite the Leyden Phial.	*Ball only.*	*Point only.*	*Apparatus reverſed.*			*Ball only.*	*Point only.*
I.	Electrometer	32	32	32	32	—		32	32
	Ball	40	39	—	30	—		29	—
	Point	76	—	71	38	—		—	39
II.	E.	28	28	28	28	—		28	28
	B.	33	36	—	29	—		28	—
	P.	72	—	66	37	—		—	38
III.	E.	25	26	26	25	repeated,	25	26	26
	B.	33	33	—	28		28	27	—
	P.	46	—	64	35		37	—	37
IV.	E.	20	20	20	20	—		20	20
	B.	21	23	—	24	—		24	—
	P.	50	—	60	26	—		—	27
V.	E.	16	16	16	16	alternately.	—	16	16
	B.	21	15	—	19		—	19	—
	P.	55	—	53	21		—	—	24
VI.	E.	13	13	13	13	—		13	13
	B.	16	11	—	14	—		15	—
	P.	44	—	42	19	—		—	22
VII.	E.	10	10	10	10	alternately.	—	10	10
	B.	11	9	—	11		—	12	—
	P.	38	—	37	19		—	—	19

Becauſe the Electrometer in the Experiments contained in the 3d Table, made at Dr. HIGGINS's with a ruſty Chain of Iron, ſtood at 21 and 23, we repeated them at Mr. PARTINGTON's with a Braſs Chain; the Reſults were as follows:

Electrometer	21	Apparatus reverſed.	23
Ball	24		25
Point	64		30

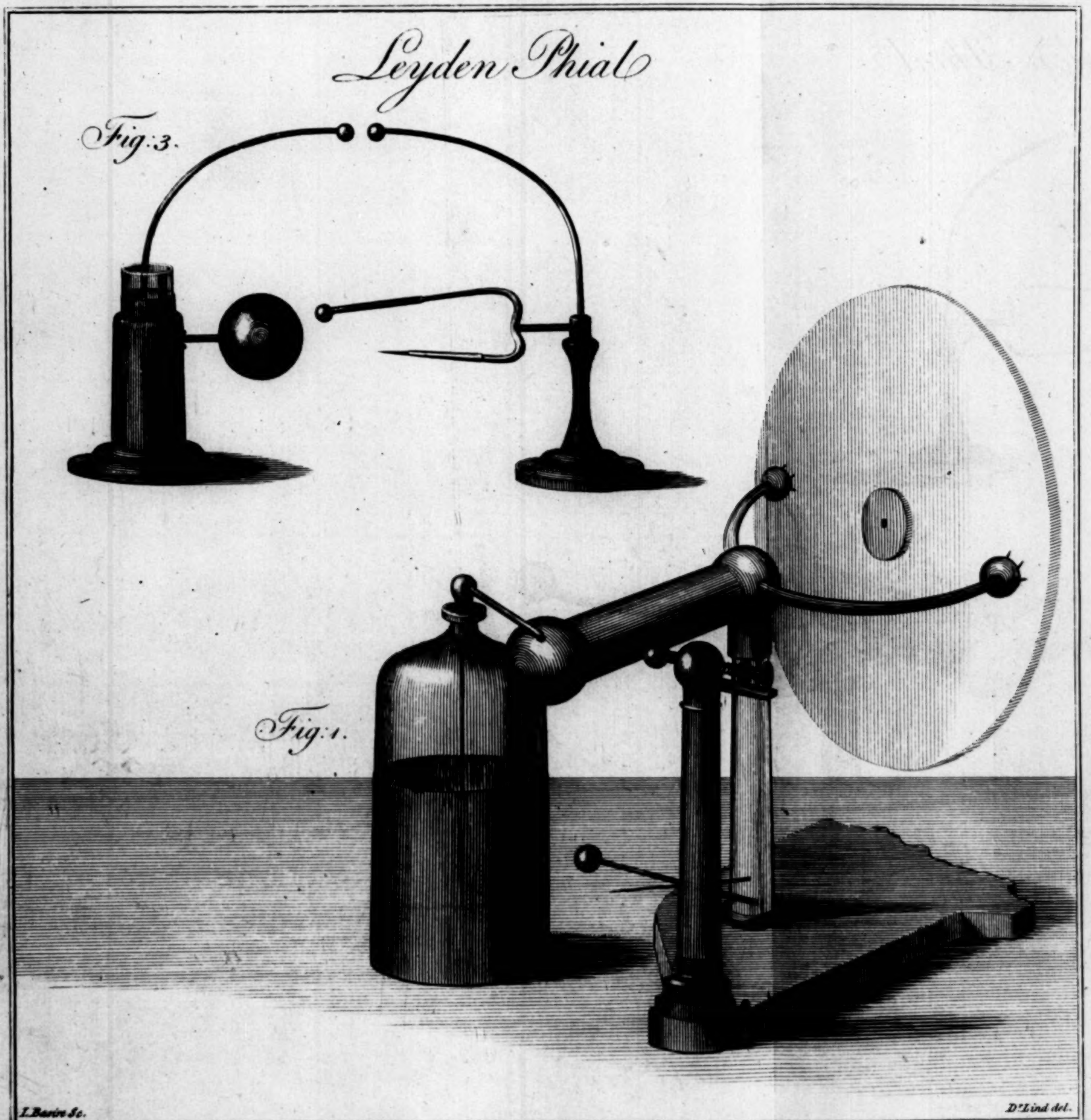
JAMES LIND.
TIBERIUS CAVALLO.
BENJAMIN WILSON.

Leyden Phial. *Fig: 2.*



A.B. The Electrometer,

In making the Experiments the Forked part of this instrument was fixed horizontally and not vertically as is here represented.


Leyden Phial
Fig:3.
Fig:1.
I. Basire Sc.
Dr. Lind del.

P. S. Having ſeen a method uſed by Mr. Cavallo to repair broken Leyden Phials, ſo as to render them again uſeful for experiments, I am glad of this opportunity to make it known, as it may be very acceptable to electricians. The method is as follows:

WHEN a coated Phial is cracked, either by a ſpontaneous diſcharge, or by any other accident, Mr. Cavallo removes the outſide coating from the fractured part, and then makes it moderately hot by holding it to the flame of a candle, and whilſt it remains hot he applies burning ſealing wax to the part, ſo as to cover the fracture entirely; taking care that the thickneſs of the wax is rather more than the thickneſs of the glaſs. Laſtly, he covers all the ſealing wax, and alſo part of the ſurface of the glaſs beyond it, with a compoſition made with four parts of bees-wax, one of reſin, one of turpentine, and a very little oil of olives; which compoſition he ſpreads upon a piece of oiled ſilk, and applies it in the manner of a plaiſter. With this method I have ſeen ſe-

veral

veral Phials ſo effectually repaired, that after being frequently charged, were at laſt broken by a ſpontaneous diſcharge, but in a different part of the glaſs.

ERRATUM.

Page 1, for 12th read 15th.